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SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L.&E. &c.
RICHARD TAYLOR, F.L.S. G.S. Astr.S. Nat.H.Mosc. &c.
RICHARD PHILLIPS, F.R.S.L.&E. F.G.S. &c.
ROBERT KANE, M.D. M.R.I.A.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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PLATE,

Illustrative of Captain JAMES's Paper on the Variegated Appearances of the New and Old Red Sandstone Systems.

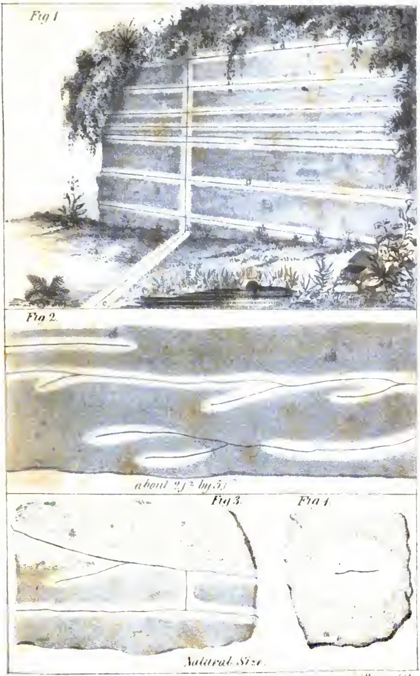
ERRATA.

VOL. XXII.

Page 464, paragraph 81, line 4, *for* cathion *read* anion.

PRESENT VOLUME.

P. 274, line 2 from the bottom, *for* B C, C D, &c., *read* B C, D E, &c.



Captn James, delt. Basire lith.

Variegated Appearances of the New and Old Red Sandstone.

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1843.

I. *Remarks on the Variegated Appearances of the New and Old Red Sandstone Systems. By* Captain JAMES, *Royal Engineers, F.G.S., &c.* *

[Illustrated by Plate I.]

HAVING in company with Sir Henry De la Beche and Professor Phillips, examined a large tract of country last year, including Herefordshire and Gloucestershire, where both the Old and New Red Sandstone systems are so well developed, I observed that that peculiarity in colour which has obtained for the New Red Sandstone group the name of Variegated or Poikilitic, was not due to any peculiarity of colour in the nature of the sand or marl deposited, nor to any peculiarity in the mode of deposition, but that it was due simply and solely to a cause coming into action since the deposition of the strata; and in point of fact this variegated appearance is produced by causes which have discharged the colour from groups of strata which were originally of a nearly uniform tint. I shall proceed to illustrate this view of the subject by sketches.

At Garden Cliff, near Westbury-on-Severn, where a beautiful section of the lower beds of the lias and the upper beds of the New Red series presents itself, I observed that the peculiar bluish-green colour (which at a distance gives the cliff the appearance of being composed of alternate strata of red sandstone or marl, and strata of this colour) was not in reality confined to the strata, but, on the contrary, that it extended two or three inches on either side of the dividing planes of the strata; and again, that the same colour appeared at about the

* Communicated by Sir Henry T. De la Beche, F.R.S., F.G.S., Conductor of the Ordnance Geological Survey.

same distance on either side the great cross lines, *a*, *b* (Plate I. fig. 1), whether a fault was produced or not, and this not only in the vertical section, but also along the strata as looking down upon them, at the foot of the cliff, at *b*, *c**. The same appearance, or rather the same fact, may be seen in the Old Red Sandstone in the cliff lower down the Severn near Purton passage; and it is almost impossible to look at this and not to see that the fissures, whether it be those between the strata, or those which intersect the strata vertically, have acted as channels through which something has been introduced to discharge the colour on either side; but if we arrive at this conclusion by looking at the matter on a large scale, the examination of the separate slabs brings the fact home to us even more strikingly, for example, in fig. 2. Looking down upon a slab of Old Red Sandstone, which I sketched in company with Professor Phillips, we see cracks extending across it and branching, and this bluish-green colour extending at a nearly equal distance on either side, following every turn and branch, whilst there is no crack or alteration in the structure of the rock at the boundary of the colour. Again, in fig. 3, in marl (and this is a sketch the size of the original), we see the same central crack along the bluish-green colour; and even this may be sometimes observed in the circular spots (fig. 4), they being the sections of pipes such as one of those in fig. 2. One might multiply such examples to any extent, but enough have been given to establish the fact, that the original red colour has been discharged or altered; and this has taken place fully to the same extent in the fine sandstones and marl of the Old Red, as it has in those of the new, though perhaps the colours are not so vivid.

May 15, 1843. HENRY JAMES, Capt. R. E.

Since writing the above, I learn from Mr. Mallet, whose scientific acquirements are so well known, that the colouring matter of these light greenish beds in the New Red Sandstone is the protoxide of iron; and he says, "If [through] a fissure in a rock containing peroxide of iron a stream of water should pass containing an earthy sulphate and organic matter, the sulphate will be decomposed, and sulphuretted hydrogen evolved, which might reduce the peroxide of iron to a lower oxide."

This seems to offer the most satisfactory explanation of the chemical action which has taken place. Earthy sulphates

* In cases where the strata are not equal to twice the distance at which the colour is discharged, the whole stratum will of course appear of the light colour, as at *c*.

abound in the New Red Sandstone series itself, as well as in the lias and other rocks above; and water from the surface of the earth passing through the strata must be constantly transporting organic matter from the surface, small in quantity perhaps in a limited time, but large enough for the observed effect when supposed to be in constant action for thousands of years; and this applies with equal force to the Old as to the New Red series, in which, as I have said before, the same effect may be observed.

May 23, 1843. H. James.

II. *On the Colouring Matters of the Persian Berries.* By Robert Kane, *M.D., M.R.I.A.**

These berries, the fruit of the dyer's buckthorn, *Rhamnus Tinctoria*, are imported from the Levant and from the south of France, for the use of dyers, to whom they furnish a yellow colour of great brilliancy, though not so permanent as some others. The appearance of the berries, as found in commerce, varies considerably; some samples, and those the most valuable, being larger, fuller, and of a light greenish-olive colour, whilst others are smaller, as if shrivelled, and dark brown in tint. The former I consider to have the appearance of being gathered before complete ripening, whilst the latter owe their altered character to being allowed to remain longer on the stem, or to having been incautiously dried.

The colouring matter in these two kinds is essentially different. The unripe berries yield but little colour to pure water, and when digested in æther give abundance of a rich golden yellow substance, to which I give the name of *chrysorhamnine*. The dark-coloured berries contain little of the substance soluble in æther, but give out to boiling water an olive-yellow material, to which, in its pure form, I give the name of *xanthorhamnine*. This substance is produced, however, only by the decomposition of the former: thus, if the unripe berries be boiled for a few minutes in water, they, when dried, yield to æther scarcely traces of *chrysorhamnine*, this principle being, by contact of air and hot water, changed into *xanthorhamnine*.

Omitting the details of methods of purification and of analysis, the properties and composition of these bodies may be expressed as follows:—

* Read before the Royal Irish Academy, Feb. 28, 1842, and now communicated by the Author.

Chrysorhamnine is of a rich golden yellow colour, of a crystalline aspect, and may be obtained in brilliant stellated tufts of short silky needles. It is but very sparingly soluble in cold water, and when boiled with water the portion which dissolves does not separate on cooling, but is found to be changed into xanthorhamnine. It dissolves in alcohol, but is not obtained by its evaporation, without being much altered. In æther, however, it dissolves abundantly, and by the spontaneous evaporation of its solution is deposited in a pure form. It has no acid reaction, but dissolves in alkaline solutions, in which, however, it appears also to be mostly altered.

Dried at 212° Fahrenheit it consisted of

	I.	II.
Carbon	58·23	57·81
Hydrogen	4·77	4·64
Oxygen	37·00	37·55
	100·00	100·00

These numbers give the formula $C_{23}\ H_{11}\ O_{11}$, by which there should be

C_{23} =	138	58·23
H_{11} =	11	4·64
O_{11} =	88	37·13
	237	100·00

On adding an alcoholic solution of chrysorhamnine to a solution of acetate of lead, a rich yellow precipitate is formed, which, when dried at 212°, was found to be expressed by the formula $C_{23}\ H_{11}\ O_{11} + 2PbO$, the numbers being as follow:—

	Theory.	Experiment.	
Carbon	138·0	29·98	29·62
Hydrogen	11·0	2·39	2·19
Oxygen	88·0	19·11	19·59
Oxide of lead	223·4	48·52	48·60
	460·4	100·00	100·00

A little water appears to have been lost in the analysis, which, however, does not affect the formula deduced.

By the decomposition of a more basic acetate of lead, a yellow precipitate is obtained, which consisted of one equivalent of chrysorhamnine united to three equivalents of oxide of lead.

The chrysorhamnine may be easily observed in its natural state of deposition in the berry; it lines the interior of the capsule-cells with a brilliant resinous-looking pale yellow and semitransparent coating.

Xanthorhamnine is formed by boiling chrysorhamnine in

water, in a capsule, so as to admit of free access of air. It dissolves with an olive-yellow colour, and on evaporating to dryness, remains as a dark, extractive-looking mass, quite insoluble in æther, but abundantly soluble in alcohol and water. It may be procured also from the berries, without previous separation of the chrysorhamnine, by similar treatment, but it is then rendered impure by a gummy substance being mixed with it. It is very difficult to determine when this substance can be considered anhydrous. Prepared by evaporation over sulphuric acid *in vacuo*, it is quite dry, and may be powdered, but if heated it liquefies below 212°, and continues giving out watery vapour until the temperature is raised to 350°, beyond which the organic matter itself cannot be heated without decomposition. On cooling it reassumes its perfectly dry aspect, and may be easily powdered.

It was hence analysed in all these stages of desiccation, with the following results. It contained—

Dried *in vacuo*.		Formula deduced.		
Carbon	34·74	C_{23} =	138	34·78
Hydrogen	6·93	H_{27} =	27	6·80
Oxygen	58·33	O_{29} =	232	58·42
	100·00		397	100·00

Dried at 212°.			Formula deduced.		
Carbon	49·97	51·20	C_{23} =	138	50·92
Hydrogen	5·18	5·28	H_{13} =	13	4·80
Oxygen	44·85	43·52	O_{15} =	120	44·28
	100·00	100·00		271	100·00

Dried in an oil-bath at 320°.		Formula deduced.		
Carbon	52·55	C_{23} =	138	52·67
Hydrogen	5·15	H_{12} =	12	4·58
Oxygen	42·30	O_{14} =	112	42·75
	100·00		262	100·00

By adding a solution of xanthorhamnine to solutions of acetate of lead, two combinations may be formed, one by neutral acetate of lead, the other by using the tribasic salt. But it is difficult to obtain either unmixed with some traces of the other, and thence the analysis of both vary a little from the true atomic constitution. Thus the tribasic salt gives—

Dried at 212°.			Formula deduced.		
Carbon	26·58		C_{23} =	138·0	26·93
Hydrogen	2·86		H_{15} =	15·0	2·93
Oxygen	25·97		O_{17} =	136·0	26·54
Oxide of lead	45·36	44·59	2.PbO =	223·4	43·60
	100·00			5124	100·00

The tribasic salt gives—

	Dried at 212°.		Formula deduced.		
Carbon . . .	21·89	22·07	C_{23} =	138·0	21·20
Hydrogen . .	3·06	2·82	H_1 =	18·0	2·76
Oxygen . . .	23·75	23·73	O_{20} =	160·0	24·57
Oxide of lead	52·30	51·38	3.PbO =	335·1	51·47
	100·00	100·00		651·1	100·00

If we consider the xanthorhamnine, as dried in the oil-bath, to be then anhydrous, the bodies analysed become

Xanthorhamnine, dry = $C_{23}\,H_{12}\,O_{14}$.
... dried at 212° = $C_{23}\,H_{12}\,O_{14}+$ Aq.
... dried *in vacuo* = $C_{23}\,H_{12}\,O_{14}+15$ Aq.

1st lead salt, $C_{23}\,H_{12}\,O_{14}+2.PbO+3$ Aq.
2nd lead salt, $C_{23}\,H_{12}\,O_{14}+3.PbO+6$ Aq.

The xanthorhamnine is thus formed by the addition of one equivalent of water and two of oxygen to the chrysorhamnine, as $C_{23}\,H_{11}\,O_{11}+HO+O_2 = C_{23}\,H_{12}\,O_{14}$. And if we were to consider the substance dried in the oil-bath at 320° still to retain an atom of water, it should be simply oxidated chrysorhamnine, being, when dry,

$$C_{23}\,H_{11}\,O_{11} + 2\,O.$$

III. *Demonstration of the Rule of Fourier.* By J. R. Young, *Esq., Professor of Mathematics in Belfast College*.*

Every thing relating to the analysis and solution of numerical equations has at length been brought under the dominion of common algebra, with the single exception of the rule which Fourier has proposed for discovering the character of a pair of roots indicated in a given interval. The investigation of this rule has hitherto involved the analytical theory of curves, or else the theorem of Lagrange on the limits of Taylor's series. It is desirable that this rule be stripped of its transcendental form, and be reduced to a level with the other general principles that now constitute the doctrine of numerical equations. It is the intention of the following proof to effect this object.

Let a, b represent the numbers which bound the doubtful interval comprehending the two roots sought. We may consider these numbers to be positive, giving rise to the following variations in the three final functions:—

$$f_2(x)\ f_1(x)\ f(x)$$

* Communicated by the Author.

$(a.)$ . . . . $+ \quad - \quad +$
$(b.)$ . . . . $+ \quad + \quad +$

Let $a + h$ be one of the intervening roots of $f(x) = 0$—the least—and $b - k$ the other: we shall assume h and k to be *real*. From common algebraical principles we have

$$f_2(a + h) = f_2(a) + f_3(a)h + f_4(a)\frac{h^2}{2} + f_5(a)\frac{h^3}{1.2.3}$$
$$+ \ldots . f_n(a)\frac{h^{n-2}}{1.2.3\ldots n-2};$$

and the right-hand member of this is the second limiting polynomial derived from

$$f_2(a)\frac{h^2}{2} + f_3(a)\frac{h^3}{2.3} + \ldots . f_n(a)\frac{h^n}{2.3\ldots n} \quad . \quad . \quad . \quad [1.]$$

these limiting polynomials being

$$f_2(a)h + f_3(a)\frac{h^2}{2} + f_4(a)\frac{h^3}{2.3} + \ldots . f_n(a)\frac{h_{n-1}}{2.3\ldots n-1} \qquad [2.]$$

$$f_2(a) + f_3(a)h + f_4(a)\frac{h^2}{2} + \ldots . f_n(a)\frac{h^{n-2}}{2.3\ldots n-2} \quad . \quad [3.]$$

The positive roots of the equations $[1.] = 0$, $[2.] = 0$, $[3.] = 0$, when written in ascending order, are known to arrange themselves as follows:—

$$\begin{array}{cccccc} 0 & 0 & a_1 & a_2 & \ldots\ldots & \\ & 0 & b_1 & b_2 & \ldots\ldots & \\ & & c_1 & c_2 & \ldots & \end{array}$$

Consequently [1.] can suffer no change of sign as h proceeds from $h = 0$ up to $h = c_1$, the least positive root of $[3.] = 0$. And a like conclusion has of course place when in [1.], [2.], [3], $-k$ is put for h*.

Now by hypothesis,

$$f(a + h) = f(a) + f_1(a)h + f_2(a)\frac{h^2}{2} + \ldots . = 0$$

$$f(b-k) = f(b) - f_1(b)k + f_2(b)\frac{k^2}{2} - \ldots . = 0.$$

And by the conclusions just established,

$$f_2(a)\frac{h^2}{2} + \ldots\ldots$$

and $$f_2(b)\frac{k^2}{2} - \ldots\ldots$$

* These inferences equally follow though a_1, a_2, &c. be imaginary.

are both *positive* quantities. Also by hypothesis $f_1(a)$ is negative, and $f_1(b)$ positive: consequently in the equality

$$-\frac{f(a)}{f_1(a)} = h + \frac{f_2(a)}{f_1(a)}\frac{h^2}{2} + \ldots.$$

the terms after h are, in the aggregate *negative*; and in

$$-\frac{f(b)}{f_1(b)} = -k + \frac{f_2(b)}{f_1(b)}\frac{k^2}{2} - \ldots.$$

the terms after $-k$ are, in the aggregate *positive*. Hence, by subtraction,

$$-\frac{f(a)}{f_1(a)} + \frac{f(b)}{f_1(b)} = h + k + \text{a negative quantity}$$

therefore, regarding only absolute numerical values,

$$\frac{f(a)}{f_1(a)} + \frac{f(b)}{f_1(b)} < h + k.$$

But $b - a$ is necessarily not less than $h + k$: consequently

$$\frac{f(a)}{f_1(a)} + \frac{f(b)}{f_1(b)} < b - a,$$

the condition which must be fulfilled whenever, as assumed above, the doubtful roots are real: and this is the criterion of Fourier.

Belfast, May 13, 1843.

IV. *On the Problem of Three Bodies.* *By the Rev.* Brice Bronwin*.

Let M, m, and m' be three bodies acting upon each other with forces as the reciprocal square of the distance; let x, y, and z be the coordinates of m, x', y', and z' those of m', both referred to M as their origin; also let $x'' = x' - x$, $y'' = y' - y$, $z'' = z' - z$ be those of m' parallel to the former, having m for their origin; and let

$$r = \sqrt{x^2+y^2+z^2},\ r' = \sqrt{x'^2+y'^2+z'^2},\ r'' = \sqrt{x''^2+y''^2+z''^2}.$$

To abridge we shall make $\mathrm{M} + m = \mu$, $\mathrm{M} + m' = \mu'$, $m + m' = \mu''$, $\mathrm{M} + m + m' = \mathrm{N}$. Then if $\mathrm{Q} = -\frac{\mu}{r} + \frac{m(xx' + yy' + zz')}{r'^3} - \frac{m'}{r''}$, the differential equations of the motion of m about M are

$$\frac{d^2x}{dt^2} + \frac{d\mathrm{Q}}{dx} = 0,\quad \frac{d^2y}{dt^2} + \frac{d\mathrm{Q}}{dy} = 0,\quad \frac{d^2z}{dt^2} + \frac{d\mathrm{Q}}{dz} = 0.$$

* Communicated by the Author.

Making x, y, z, and m change places with x', y', z' and m';

$$\frac{d^2 x'}{d t^2} + \frac{d Q'}{d x'} = 0, \quad \frac{d^2 y'}{d t^2} + \frac{d Q'}{d y'} = 0, \quad \frac{d^2 z'}{d t^2} + \frac{d Q'}{d z'} = 0$$

for the motion of m. And similarly for the motion of m' about m;

$$\frac{d^2 x''}{d t^2} + \frac{d Q''}{d x''} = 0, \frac{d^2 y''}{d t^2} + \frac{d Q''}{d y''} = 0, \frac{d^2 z''}{d t^2} + \frac{d Q''}{d z''} = 0. \quad \text{(A.)}$$

By substituting for Q, Q', and Q'' their values, we shall easily find

$$\left.\begin{aligned}
\frac{x\,d^2 y - y\,d^2 x}{d t^2} &= (x' y - x y') \cdot \left(\frac{m'}{r'^3} - \frac{m'}{r''^3}\right), \\
\frac{x\,d^2 z - z\,d^2 x}{d t^2} &= (x' z - x z') \cdot \left(\frac{m'}{r'^3} - \frac{m'}{r''^3}\right), \\
\frac{y\,d^2 z - z\,d^2 y}{d t^2} &= (y' z - y z') \cdot \left(\frac{m'}{r'^3} - \frac{m'}{r''^3}\right); \\
\frac{x'\,d^2 y' - y'\,d^2 x'}{d t^2} &= (x' y - x y') \cdot \left(\frac{m}{r''^3} - \frac{m}{r^3}\right), \\
\frac{x'\,d^2 z' - z'\,d^2 x'}{d t^2} &= (x' z - x z') \cdot \left(\frac{m}{r''^3} - \frac{m}{r^3}\right), \\
\frac{y'\,d^2 z' - z'\,d^2 y'}{d t^2} &= (y' z - y z') \cdot \left(\frac{m}{r''^3} - \frac{m}{r^3}\right); \\
\frac{x''\,d^2 y'' - y''\,d^2 x''}{d t^2} &= (x' y - x y') \cdot \left(\frac{M}{r^3} - \frac{M}{r'^3}\right), \\
\frac{x''\,d^2 z'' - z''\,d^2 x''}{d t^2} &= (x' z - x z') \cdot \left(\frac{M}{r^3} - \frac{M}{r'^3}\right), \\
\frac{y''\,d^2 z'' - z''\,d^2 y''}{d t^2} &= (y' z - y z') \cdot \left(\frac{M}{r^3} - \frac{M}{r'^3}\right).
\end{aligned}\right\} \ldots \text{(B.)}$$

From the last we derive without difficulty,

$$\left.\begin{aligned}
\frac{x\,d^2 y - y\,d^2 x}{x\,d^2 z - z\,d^2 x} &= \frac{x'\,d^2 y' - y'\,d^2 x'}{x'\,d^2 z' - z'\,d^2 x'} = \frac{x''\,d^2 y'' - y''\,d^2 x''}{x''\,d^2 z'' - z''\,d^2 x''} \\
\frac{x\,d^2 y - y\,d^2 x}{y\,d^2 z - z\,d^2 y} &= \frac{x'\,d^2 y' - y'\,d^2 x'}{y'\,d^2 z' - z'\,d^2 y'} = \frac{x''\,d^2 y'' - y''\,d^2 x''}{y''\,d^2 z'' - z''\,d^2 y''}.
\end{aligned}\right\} \text{(C.)}$$

From these two, dividing the one by the other, we obtain a third set.

From the same source also we find

$$\left.\begin{aligned}
(x' z - z' x)\,(x\,d^2 y - y\,d^2 x) &= (x' y - x y')\,(x\,d^2 z - z\,d^2 x), \\
(x' z - z' x)\,(x'\,d^2 y' - y'\,d^2 x') &= (x' y - y' x)\,(x'\,d^2 z' - z'\,d^2 x'), \\
(x' z - z' x)\,(x''\,d^2 y'' - y''\,d^2 x'') &= (x' y - y' x)\,(x''\,d^2 z'' - z''\,d^2 x''), \\
(y' z - z' y)\,(x\,d^2 y - y\,d^2 x) &= (x' y - y' x)\,(y\,d^2 z - z\,d^2 y),
\end{aligned}\right\} \text{(D.)}$$

and several others.

With equal ease we find from (B.), integrating results,

$$\left.\begin{aligned}
&\mathrm{M}m\frac{x\,dy-y\,dx}{dt}+\mathrm{M}m'\frac{x'\,dy'-y'\,dx'}{dt}+mm'\frac{x''\,dy''-y''\,dx''}{dt}=c,\\
&\mathrm{M}m\frac{x\,dz-z\,dx}{dt}+\mathrm{M}m'\frac{x'\,dz'-z'\,dx'}{dt}+mm'\frac{x''\,dz''-z''\,dx''}{dt}=c',\\
&\mathrm{M}m\frac{y\,dz-z\,dy}{dt}+\mathrm{M}m'\frac{y'\,dz'-z'\,dy'}{dt}+mm'\frac{y''\,dz''-z''\,dy''}{dt}=c''.
\end{aligned}\right\}\text{(E.)}$$

These last are known.

Make $\frac{m\,x+m'\,x'}{m+m'}=u,\ \frac{m\,y+m'\,y'}{m+m'}=v,\ \frac{m\,z+m'\,z'}{m+m'}=w;$ u, v, and w will be the coordinates of the centre of gravity of m and m'. To simplify, let $x''=x'-x=u'$, $y''=y'-y=v'$, $z''=z'-z=w$; we find $x=\frac{\mu''\,u-m'\,u'}{\mu''}$, $x'=\frac{\mu''\,u+m\,u'}{\mu''}$, &c. Substituting the values of x, y, and z, x', y', and z', and their differentials in (E.), there results

$$\left.\begin{aligned}
&\mathrm{M}\,\mu''\frac{u\,dv-v\,du}{dt}+\frac{\mathrm{N}\,m\,m'}{\mu''}\ \frac{u'\,dv'-v'\,du'}{dt}=c,\\
&\mathrm{M}\,\mu''\frac{u\,dw-w\,du}{dt}+\frac{\mathrm{N}\,m\,m'}{\mu''}\ \frac{u'\,dw'-w'\,du'}{dt}=c',\\
&\mathrm{M}\,\mu''\frac{v\,dw-w\,dv}{dt}+\frac{\mathrm{N}\,m\,m'}{\mu''}\ \frac{v'\,dw'-w'\,dv'}{dt}=c''.
\end{aligned}\right\}\ .\ \text{(F.)}$$

Thus the area described by the centre of gravity of m and m' about M, multiplied by $\mathrm{M}\,(m+m')$, added to the area described by m' about m, multiplied by $(\mathrm{M}+m+m')\,\frac{m\,m'}{m+m'}$; on each of the coordinate planes is a constant quantity. Two other similar sets of equations exist relative to the centre of gravity of M and m, and of M and m'.

To abridge, we make $\frac{x\,dy-y\,dx}{dt}=c_1,\ \frac{x\,dz-z\,dx}{dt}=c_2,$

$$\frac{y\,dz-z\,dy}{dt}=c_3;\ \ \frac{x'\,dy'-y'\,dx'}{dt}=c'_1,\ \&\text{c.};$$

$$\frac{x''\,dy''-y''\,dx''}{dt}=c''_1,\ \&\text{c.}$$

The following are identical:—

$$\left.\begin{aligned}
&c_3\,x-c_2\,y+c_1\,z=0,\ \ c'_3\,x'-c'_2\,y'+c'_1\,z'=0,\\
&c''_3\,x''-c''_2\,y''+c''_1\,z''=0,\ \ x\,dc_3-y\,dc_2+z\,dc_1=0,\\
&x'\,dc'_3-y'\,dc'_2+z'\,dc'_1=0,\ \ x''\,dc''_3-y''\,dc''_2+z''\,dc''_1=0
\end{aligned}\right\}\ \text{(G.)}$$

From the last we find by means of (C.),

$$\left.\begin{array}{l} x\,d c'_3 - y\,d c'_2 + z\,d c'_1 = 0,\ x'\,d c_3 - y'\,d c_2 + z'\,d c_1 = 0, \\ x\,d c''_3 - y\,d c''_2 + z\,d c''_1 = 0,\ x'\,d c''_3 - y'\,d c''_2 + z'\,d c''_1 = 0, \\ x''\,d c_3 - y''\,d c_2 + z''\,d c_1 = 0,\ x''\,d c'_3 - y''\,d c'_2 + z''\,d c'_1 = 0. \end{array}\right\} \text{(H.)}$$

These may be found immediately from (B.). We may find the corresponding equations of the first order from (E.). They are

$$\left.\begin{array}{l} \mathrm{N\,M}m\,(c_3 x' - c_2 y' + c_1 z') = \mu\,(c'' x' - c' y' + c z') - m\,(c'' x - c' y + c z), \\ \mathrm{N\,M}m'(c'_3 x - c'_2 y + c'_1 z) = \mu'(c'' x - c' y + c z) - m'(c'' x' - c' y' + c z'), \\ \mathrm{N\,M}m(c_3 x'' - c_2 y'' + c_1 z'') = \mu\,(c'' x' - c' y' + c z') - m\,(c'' x - c' y + c z), \\ \mathrm{N}m\,m'(c''_3 x - c''_2 y + c''_1 z) = m'(c'' x' - c' y' + c z') + m(c'' x - c' y + c z), \end{array}\right\} \text{(I.)}$$

and two others. The second members, where c'', c', and c are constant, manifest how these equations are formed.

We might derive from (B.) many other curious results, but we shall only notice the following:—

$$\left.\begin{array}{l} \mathrm{M}\,m\,\dfrac{d\,c_1}{r^3} + \mathrm{M}\,m'\,\dfrac{d c'_1}{r'^3} + m\,m'\,\dfrac{d\,c''_1}{r''^3} = 0, \\ \mathrm{M}\,m\,\dfrac{d\,c_2}{r^3} + \mathrm{M}\,m'\,\dfrac{d c'_2}{r'^3} + m\,m'\,\dfrac{d\,c''_2}{r''^3} = 0, \\ \mathrm{M}\,m\,\dfrac{d\,c_3}{r^3} + \mathrm{M}\,m'\,\dfrac{d c'_3}{r'^3} + m\,m'\,\dfrac{d\,c''_3}{r''^3} = 0. \end{array}\right\} \ .\ .\ \text{(K.)}$$

However convenient the perturbating function of Lagrange may be for the purposes to which it is applied, it is not adapted to the finding of integral equations of the first order.

Make $\mathrm{R} = \dfrac{\mathrm{M}\,m}{r} + \dfrac{\mathrm{M}\,m'}{r'} + \dfrac{m\,m'}{r''}$, and the equations (A.) may be put under the following form:—

$$\left.\begin{array}{l} \mathrm{M}\,m\,\dfrac{d^2 x}{d\,t^2} = \mu\,\dfrac{d\,\mathrm{R}}{d\,x} + m\,\dfrac{d\,\mathrm{R}}{d\,x'},\ \mathrm{M}\,m\,\dfrac{d^2 y}{d\,t^2} = \mu\,\dfrac{d\,\mathrm{R}}{d\,y} + m\,\dfrac{d\,\mathrm{R}}{d\,y'}, \\ \mathrm{M}\,m\,\dfrac{d^2 z}{d\,t^2} = \mu\,\dfrac{d\,\mathrm{R}}{d\,z} + m\,\dfrac{d\,\mathrm{R}}{d\,z'};\ \mathrm{M}\,m'\,\dfrac{d^2 x'}{d\,t^2} = \mu'\,\dfrac{d\,\mathrm{R}}{d\,x'} + m'\,\dfrac{d\,\mathrm{R}}{d\,x}, \\ \mathrm{M}\,m'\,\dfrac{d^2 y'}{d\,t^2} = \mu'\,\dfrac{d\,\mathrm{R}}{d\,y'} + m'\,\dfrac{d\,\mathrm{R}}{d\,y},\ \mathrm{M}\,m'\,\dfrac{d^2 z'}{d\,t^2} = \mu'\,\dfrac{d\,\mathrm{R}}{d\,z'} + m'\,\dfrac{d\,\mathrm{R}}{d\,z}. \end{array}\right\} \text{(L.)}$$

These may be put under the following form:—

$$\mathrm{M}\,m\,\frac{d^2 x}{d\,t^2} + m\,m'\,\frac{d^2 x - d^2 x'}{d\,t^2} = \mathrm{N}\,\frac{d\,\mathrm{R}}{d\,x},$$

$$\mathrm{M}\,m'\,\frac{d^2 x'}{d\,t^2} - m\,m'\,\frac{d^2 x - d^2 x'}{d\,t^2} = \mathrm{N}\,\frac{d\,\mathrm{R}}{d\,x'},$$

$$M\,m\frac{d^2y}{dt^2}+m\,m'\frac{d^2y-d^2y'}{dt^2}=N\frac{dR}{dy},$$

$$M\,m'\frac{d^2y'}{dt^2}-m\,m'\frac{d^2y-d^2y'}{dt^2}=N\frac{dR}{dy'},$$

$$M\,m\frac{d^2z}{dt^2}+m\,m'\frac{d^2z-d^2z'}{dt^2}=N\frac{dR}{dz},$$

$$M\,m'\frac{d^2z'}{dt^2}-m\,m'\frac{d^2z-d^2z'}{dt^2}=N\frac{dR}{dz'}.$$

Whence we easily obtain

$$M\,m\frac{dx^2+dy^2+dz^2}{dt^2}+M\,m'\frac{dx'^2+dy'^2+dz'^2}{dt^2}$$

$$+m\,m'\frac{dx''^2+dy''^2+dz''^2}{dt^2}-2\,N\,R=b,$$

the result being integrated, and x'' put for $x'-x$, &c.

If we eliminate dx, dy, dz, dx', &c. by their values in u, v, and w; u', v', w', as in (F.), we shall have

$$M\mu''\frac{du^2+dv^2+dw^2}{dt^2}+\frac{N m m'}{\mu''}\,\frac{du'^2+dv'^2+dw'^2}{dt^2}-2\,N R=b.\text{(M.)}$$

Thus the square of the velocity of the centre of gravity of m and m' about M, multiplied by M $(m+m')$, added to the square of the velocity of m' about m, multiplied by $m\,m'\dfrac{M+m+m'}{m+m'}$, is equal to 2 N R plus a constant. Two other similar formulæ may be found relative to the centres of gravity of M and m, and of M and m'

By the nature of the function R, we have

$$x\frac{dR}{dy}-y\frac{dR}{dx}+x'\frac{dR}{dy'}-y'\frac{dR}{dx'}=0,$$

$$x\frac{dR}{dz}-z\frac{dR}{dx}+x'\frac{dR}{dz'}-z'\frac{dR}{dx'}=0,$$

$$y\frac{dR}{dz}-z\frac{dR}{dy}+y'\frac{dR}{dz'}-z'\frac{dR}{dy'}=0.$$

By the aid of these properties we may easily derive from (L.), or the equations following them, the equations (E.) relative to the areas described.

We find from (A.),

$$M\,m\frac{x\,d^2x+y\,d^2y+z\,d^2z}{dt^2}+M\,m'\frac{x'\,d^2x'+y'\,d^2y'+z'\,d^2z'}{dt^2}$$

$+ m\, m' \frac{x''\, d^2 x'' + y''\, d^2 y'' + z''\, d^2 z''}{d t^2} + \mathrm{N}\, \mathrm{R} = 0.$ If we combine this with the equation immediately preceding (M.), we have

$$\left.\begin{aligned} &\mathrm{M}\, m \frac{d^2 (r^2)}{d t^2} + \mathrm{M}\, m' \frac{d^2 (r'^2)}{d t^2} + m\, m' \frac{d^2 (r''^2)}{d t^2} \\ &- 2\, \mathrm{N} \left(\frac{\mathrm{M}\, m}{r} + \frac{\mathrm{M}\, m'}{r'} + \frac{m\, m'}{r''}\right) = 2\, b. \end{aligned}\right\} \quad . \; . \; . \quad (\mathrm{N.})$$

If ϱ be the distance of the centre of gravity of m and m' from M, we have $m\, r^2 + m'\, r'^2 = \mu''\, \varrho^2 + \frac{m\, m'}{\mu''} r''^2$. By this eliminating $d^2 (r^2)$ from (N.), there results

$$\left.\begin{aligned} &\mathrm{M}\, \mu'' \frac{d^2 (\varrho^2)}{d t^2} + \frac{\mathrm{N}\, m\, m'}{\mu''} \frac{d^2 (r''^2)}{d t^2} \\ &- 2\, \mathrm{N} \left(\frac{\mathrm{M}\, m}{r} + \frac{\mathrm{M}\, m'}{r'} + \frac{m\, m'}{r''}\right) = 2\, b. \end{aligned}\right\} \quad . \quad (\mathrm{O.})$$

Let $\mathrm{P} = \frac{1}{r} + \frac{1}{r'} - \frac{1}{r''}$; its properties are

$$x \frac{d \mathrm{P}}{d y} - y \frac{d \mathrm{P}}{d x} + x' \frac{d \mathrm{P}}{d y'} - y' \frac{d \mathrm{P}}{d x'} = 0,$$

$$x \frac{d \mathrm{P}}{d z} - z \frac{d \mathrm{P}}{d x} + x' \frac{d \mathrm{P}}{d z'} - z' \frac{d \mathrm{P}}{d x'} = 0,$$

$$y \frac{d \mathrm{P}}{d z} - z \frac{d \mathrm{P}}{d y} + y' \frac{d \mathrm{P}}{d z'} - z' \frac{d \mathrm{P}}{d y'} = 0.$$

Equations (A.) will be replaced by

$$\frac{d^2 x}{d t^2} + \frac{\mu\, x}{r^3} = m' \frac{d \mathrm{P}}{d x}, \quad \frac{d^2 y}{d t^2} + \frac{\mu\, y}{r^3} = m' \frac{d \mathrm{P}}{d y}, \quad \frac{d^2 z}{d t^2} + \frac{\mu\, z}{r^3} = m' \frac{d \mathrm{P}}{d z};$$

$$\frac{d^2 x'}{d t^2} + \frac{\mu'\, x'}{r'^3} = m \frac{d \mathrm{P}}{d x'}, \quad \frac{d^2 y'}{d t^2} + \frac{\mu'\, y'}{r'^3} = m \frac{d \mathrm{P}}{d y'}, \quad \frac{d^2 z'}{d t^2} + \frac{\mu'\, z'}{r'^3} = m \frac{d \mathrm{P}}{d z'}.$$

From these, by the aid of the foregoing properties of P, we without difficulty find

$x\, d\, c'_3 - y\, d\, c'_2 + z\, d\, c'_1 = 0, \quad x'\, d\, c_3 - y'\, d\, c_2 + z'\, d\, c_1 = 0.$

These we have before found in a different manner. They are only introduced here to give an example of the use of different perturbating functions.

On some occasions perhaps the following form may be used with advantage: $\mathrm{V} = \frac{m'}{r'} - \frac{m'}{r''}$;

$$\frac{d^2 x}{d t^2} + \frac{\mu\, x}{r^3} = \frac{d \mathrm{V}}{d x}, \quad \frac{d^2 y}{d t^2} + \frac{\mu\, y}{r^3} = \frac{d \mathrm{V}}{d y}, \quad \frac{d^2 z}{d t^2} + \frac{\mu\, z}{r^3} = \frac{d \mathrm{V}}{d z}.$$

But V will not serve for the equations of the disturbing body. If we wish to find integral equations of the first order, we should have a perturbating function which will serve for two at least of the three sets of equations (A.).

My paper on M. Jacobi's Theory of Elliptic Functions, printed in the Number of this Journal for the present month, is not very intelligible. I wish, therefore, to subjoin a few words in the hope of making it plainer. The expressions sin *a m n ω*, cos *a m n ω*, mean sine of amplitude of *n ω*, cosine of amplitude of *n ω*. So also s *a u*, c *a u* mean sine and cosine of the amplitude of *u*. They should have been printed thus:—sin am *n ω*, cos am *n ω*, s *a u*, c *a u*. These faults run through the paper. Moreover the *a m* and *a* for amplitude should not have been in italics, as italics appear to denote quantity only.

Denby, near Huddersfield, April 11, 1843.

V. *On the Sugar of the Eucalyptus.*

By James F. W. Johnston, *Esq., M.A., F.R.S.**

IN Van Diemen's Land a species of sugar or manna falls in drops or rounded opaque tears from several species of *Eucalyptus*. This is collected in considerable quantity, but it is doubtful still I believe whether it is a natural exudation of the trees from which it falls, or, like the different kinds of honey-dew in our own country, is the consequence of punctures made by insects.

I am indebted for a portion of this manna to Sir W. Jackson Hooker, to whom also I owe the above information regarding its origin. It is soft, slightly yellowish, opaque, is inferior in sweetness to cane-sugar or to ordinary manna, and is in small, rounded, slightly cohering masses. Æther extracts from it only a minute portion of wax, alcohol leaves behind only a small quantity of gum, while water dissolves it without sensible residue.

The aqueous solution crystallizes on evaporation in minute radiating prisms and prismatic needles which form rounded masses having a crystalline structure. It is obtained however from water in distinct crystals with much greater difficulty than from its solution in ordinary alcohol. In boiling alcohol it dissolves in considerable quantity, and is in a great measure precipitated in beautiful white but minute prismatic crystals as the solution cools. It not unfrequently deposits itself also in the form of a white hard and solid crust on the bottom and sides of the bottle into which the hot solution is filtered.

This sugar as it crystallizes from the alcoholic solution has

* Communicated by the Chemical Society; having been read December 20, 1842.

the same constitution as grape-sugar, $C_{12}\ H_{14}\ O_{14}$, or $C_{24}\ H_{28}\ O_{28}$, but it differs from grape-sugar in its appearance, in its relations to alcohol as above described, in the ease with which it can be obtained in a pure crystallized form, and in its relations to heat.

When suddenly heated at once to 200° or 212°, it melts and loses 5 atoms of water, whereas grape-sugar loses only *four*. But if it be first gradually heated and kept for two or three hours at 180° only, it will part with seven atoms of water *without melting*. In that respect it resembles a salt, which if heated suddenly will melt in its water of crystallization, but by a cautious regulation of the heat may be dried without undergoing fusion. If once melted, this sugar may be kept for several hours at 212° without losing *much* more than the five atoms, and it must be raised to 240° or 250° before it parts with the whole seven, and in every case in which I have made the experiment has even assumed a brown colour, owing to incipient decomposition before the seven atoms have been altogether removed.

When the seven atoms have been driven off by a heat not higher than 200°, the dry powder may be heated to 280°, when it begins to fuse, and may be kept for several hours at 300° without further loss or any change of colour.

After being thus heated the sugar attracts moisture rapidly from the air, and if left over night in a damp room it will assume the form of transparent globules of syrup, which gradually crystallize into colourless radiated masses having the original weight of the portion of sugar experimented upon. We may conclude therefore that the seven atoms are altogether water of crystallization.

When mixed with oxide of lead moistened with water and then gradually dried and heated to 300°, it *appeared* to lose two additional atoms of water without undergoing decomposition; but when exposed to the air on cooling, the mixture rapidly attracts water again from the air. When this mixture after thus heating is boiled with distilled water and thrown upon the filter, a solution of sugar passes through in which hydrosulphurets detect no trace of lead.

The following formulæ exhibit the constitution of this sugar and the loss of weight it undergoes at different temperatures:—

		Loss by experiment, per cent.
Crystallized sugar before or after heating . . .	$C_{24}\ H_{21}\ O_{21} + 7\ HO$	
Fused at 212° to 220° .	$C_{24}\ H_{21}\ O_{21} + 2\ HO$	11·23
Dried without fusion between 180° and 300°	$C_{24}\ H_{21}\ O_{21}$	15·88

		Loss by experiment, per cent.
Dried at 260° to 300° with oxide of lead . .	$C_{24}\ H_{19}\ Pb_2\ O_{21}$?	20·82?
This again exposed to the air became . . .	$C_{24}\ H_{19}\ Pb_2\ O_{21} + 7\,HO$?	

This sugar, in its relations to alcohol, in the ease and readiness with which it crystallizes from an alcoholic solution, and in the appearance of its crystals, has much resemblance to manna-sugar (Mannite). It is more soluble however in boiling alcohol than mannite, and is therefore obtained in larger quantity on the cooling of the alcohol in which it has been dissolved by the aid of heat. Mannite also, if heated gradually, may be raised to 300° (I do not know how much higher) without either melting or undergoing any loss of weight.

Eucalyptus-sugar gives a precipitate of a slightly brownish tinge with caustic baryta; and a white precipitate is also obtained by mixing it with a solution of ammoniacal trisacetate of lead. This salt of lead I am at present preparing for analysis, and I hope to have the honour of submitting the results to the Society at a future meeting. In the mean time the formulæ presented in this notice must be considered as open to correction.

VI. *On Palladium—Its Extraction, Alloys, &c.* *By* William John Cock, *Esq.**

THIS metal was discovered by Dr. Wollaston in the year 1803 †, as one of the alloys of native platinum, which for some time after this discovery appears to have been considered the only source of palladium; and as the quantity of the latter metal so alloying the native platinum is very small, it was then considered as a very rare metal: of late years, however, the importation into this country from Brazil of gold dust, alloyed with palladium, has occasioned a much more extensive supply of this metal, as it exists in some specimens of gold dust to the extent of 5 or 6 per cent., and in one instance (that of the gold from the Candonga mine) it constitutes the only alloy of the gold.

The operation of refining is conducted in the following manner:—The gold dust is fused in charges of about 7 lbs.

* Communicated by the Chemical Society; having been read January 3, 1843.

† Dr. Wollaston's original paper on Palladium, reprinted from the Philosophical Transactions, will be found in Phil. Mag. S. 1. vol. xx. p. 163; see also vol. xv. p. 287.—Edit.

troy, with its own weight of silver and a certain quantity of nitrate of potash; the effect of this fusion is to remove all earthy matter, and the greater part of the base metals contained in the gold dust and in the silver melted with it. The fused mixture is cast into ingot moulds, and when cooled, the flux or scoria (containing the oxides of the base metals and the earthy matter, combined with the potash of the nitre) is detached. Two of the bars thus obtained are then remelted in a plumbago crucible, with such an addition of silver as will afford an alloy containing one-fourth its weight of pure gold, and which being first well-stirred to insure a complete mixture, is poured through a perforated iron ladle into cold water, and thus very finely granulated; it is then ready for the process of parting. For this purpose about 25 lbs. of the granulated alloy is placed in a porcelain jar, upon a heated sand-bath, and subjected to the action of about 25 lbs. of pure nitric acid, diluted with its own bulk of water: after the action of this quantity of acid, the parting of the gold is very nearly effected; but to remove the last portions of silver, &c., about 9 or 10 lbs. of strong nitric acid is boiled upon the gold for two hours. It is then completely refined, and after being washed with hot water is dried and melted into bars containing 15 lbs. each.

The nitrous acid gas, and the vapour of nitric acid arising during the above process, are conducted by glass pipes (connected with the covers of the jars) into a long stone-ware pipe, one end of which slopes downwards into a receiver for the condensed acid, the other end being inserted into the flue for the purpose of carrying off the uncondensed gas.

The nitrate of silver and palladium obtained as above is carefully decanted into large pans, containing a sufficient quantity of solution of common salt to effect the precipitation (as a chloride) of the whole of the silver, the palladium and copper remaining in solution in the mother liquor, which is drawn off, and when clear is run off, together with the subsequent washings from the chloride of silver, into wooden vessels, and the metallic contents are then separated in the form of a black powder, by precipitation with sheet zinc, assisted by sulphuric acid.

The chloride of silver, when washed clean, is reduced by the addition of granulated zinc washed on the filter with boiling water, dried and melted in plumbago crucibles, without the addition of any flux.

From the black powder obtained as above, the palladium is extracted by resolution in nitric acid and super-saturation with ammonia, by which the oxides of palladium and copper

are first precipitated and then redissolved, while those of iron, lead, &c., remain insoluble. To the clear ammoniacal solution, muriatic acid is then added in excess, which occasions a copious precipitation of the yellow ammonio-chloride of palladium, from which, after sufficiently washing it with cold water and ignition, pure metallic palladium is obtained. The mother liquor and washings contain all the copper and some palladium, which are recovered by precipitation with iron.

Pure palladium is of a greyish-white colour, rather darker than that of platinum; it is both malleable and ductile, though inferior in those qualities to pure platinum; its specific gravity is 11·3, which may be raised by hammering or rolling to 11·8. When perfectly pure it cannot be fused even in small quantities in an ordinary blast furnace, but may be brought into such a state of agglutination as to bear laminating or drawing into wire.

It may be completely fused by means of oxygen gas, and being kept some time fused, is said to burn with the production of brilliant sparks; it is not tarnished by exposure to sulphuretted hydrogen, nor oxidated by the air at the ordinary temperature, or at a bright red heat; but it has the singular property of becoming oxidated by exposure to air at a dull red heat, the surface becoming coloured in the same manner as iron or steel; and by continuing the process cautiously for some time, the metal becomes coated with a brittle crust of oxide of a brown colour; this oxide is, however, reduced by a temperature very little higher than that necessary for its formation; and the surface of the metal regains its original colour upon being heated to a bright red, and cooled out of contact with the air.

It is with difficulty soluble in nitric acid when pure and fused, or in a state of aggregation, but is readily so when alloyed to some extent with silver or copper, and still more so when in the form of the black powder above referred to, in which state it is also soluble with the aid of heat in sulphuric and muriatic acids; but its proper solvent is nitro-muriatic acid, which, if it be not very much alloyed with silver, dissolves it readily.

It is of all the metals that which has the greatest affinity for cyanogen; and by means of cyanide of mercury, it may be separated from all its solutions.

It may be alloyed so as to be malleable with gold, silver and copper, several of its alloys with the two latter metals being of great use in the arts from their hardness and elasticity, and non-liability to rust or tarnish. When added to gold or copper, it whitens both those metals in a very great

degree, about 20 per cent. being sufficient in either case to destroy the colour of those metals.

The uses to which the alloys of palladium have been applied, are for the points of pencil-cases, for lancets for vaccination, for the graduated scales of instruments, as a substitute for gold in dental surgery, or for any purpose where strength and elasticity, or the property of not tarnishing, is required.

VII. *On the Formation of Fat in the Animal Body.* *By* Justus Liebig, *Ph.D., M.D., F.R.S., &c.**

In my published work on 'Organic Chemistry, in its application to Physiology and Pathology,' I have endeavoured to explain the nutrition of the human and animal organism, according to the present state of organic chemistry. I have pointed out the relation between the nitrogenous food and the nitrogenous constituents of animal bodies, and have considered the non-nitrogenous constituents of the food as the means of the formation of the non-nitrogenous constituents of animals.

The circumstance, that the large class of carnivorous animals do not take any sugar, starch, or gum in their food, leads of itself to the opinion that these substances are not required for proper nourishment, namely, for the formation of blood; and as it appears from the analysis of plants containing nitrogen, that they possess a similar composition to the substances of the blood, it follows also that in the bodies of herbivorous and graminivorous animals, the carbon of the sugar, gum and starch cannot be applied to the formation of the blood. The nitrogen of the nitrogenous ingredients of the food is therefore in a state of combination, in which the elements necessary for the production of the albumen are already present both in number and relative proportions; in the food of the graminivorous animals, we know after all of no other compound, which can supply nitrogen to starch, sugar, or gum, for the production of albumen.

As sugar, gum and starch, in their normal state, disappear in the vital processes of graminivorous animals, and as they are given out of their bodies as carbonic acid and water, it follows from such a conversion that they serve by means of the respiration for the production of animal heat.

The disappearance of fat in animals in consequence of

* Translated from the German original by Mr. E. F. Teschemacher, and communicated by the Chemical Society; having been read January 3, 1843. On the subject of this paper see a translation of M. Dumas's memoir On the Chemical Statics of Organized Beings, in Phil. Mag. S. 3. vol. xix.

disease, or of an increased absorption of oxygen, and its being given out in the form of carbonic acid and water, is a proof that that non-nitrogenous body is converted to the same use as sugar, gum, or starch in animal bodies, and for want of other non-nitrogenous food is applied to the respiration.

The further consideration, that the flesh of the carnivorous, which of all animals eat most fat, contains no fat, and is not eatable; that the fat in the bodies of graminivorous animals increases when the process of respiration, and with it the absorption of oxygen diminishes, through a want of exercise or an increase of temperature, leads to the conclusion that the fat has its origin in the non-nitrogenous food, the carbon remaining in the body in the form of fat when there is a deficiency of the necessary quantity of oxygen to convert it into carbonic acid.

Supported by the example of what certainly takes place in the processes of fermentation and putrefaction, in which sugar and starch, by giving out oxygen or carbonic acid, form new combinations, which, like æther and fusel oil, more resemble fat in their properties than any other known compounds, I have endeavoured to trace out the formation of fat, on the supposition that the carbon of non-azotized substances remains in the animal body in the form of fat.

According to my statement, the fat consequently originates from the non-nitrogenous constituents of the food: let us suppose from sugar, then this must have undergone a chemical change in conformity with my proposition.

The formation of wax from honey which contains none, in the body of the bee, of which, from the experiments of M. Grundlach of Cassel, there can be no doubt, appears to remove every objection to the possibility of such an action taking place.

I never had the least idea of defending in my book the opinion, or even of expressing it, that the fat which was taken in the food of animals did not contribute to increase the quantity of fat in their bodies; but I was not aware of any supply of butter in the grass which is daily consumed by cows, or of tallow, of lard, or goose-fat in potatoes, barley and oats; in the analyses of these substances as at present given, they contain only waxy substances, and that in such a small quantity that I consider the formation of fat could not be attributed to it.

These ideas concerning the origin of fat in animal bodies took a new direction from a note which M. Dumas appended in the *Annales de Chimie* (new Series, vol. iv. p. 208) to my treatise on the nitrogenous food of the vegetable kingdom;

in this note M. Dumas says,—" M. Liebig is of opinion that graminivorous animals *produce* fat out of sugar and starch, while MM. Dumas and Boussingault consider it as a fixed rule, that animals, of whatever kind, produce neither fat nor any other alimentary substance; that they receive from the vegetable kingdom all their aliments, whether it be sugar, starch, or fat.

" Were the proposition of M. Liebig founded upon fact, the general formula of chemical equivalents of both kingdoms, as defined by MM. Dumas and Boussingault, would be false. But the commission on gelatine has dispelled all doubt, that the animals which eat fat are the only ones in which fat is found to accumulate in the tissues."

The origin of fatty compounds in animal bodies has, through this note, become a question of dispute.

As far as regards myself, I have neither time nor inclination to engage in it; the object of my observations was to leave no doubt of the physiological importance of the fat of animal bodies, as regards the process of respiration. In this view MM. Dumas and Boussingault agree with me.

I think it now right to explain the reasons which induced me to consider that little or no increase of fat in animal bodies was to be ascribed to the ingredients of the food containing fat, consumed by graminivorous animals.

The food which, according to the experience of physicians, has a decided influence on the formation of fat in animal bodies, is that which is richest in starch, sugar, and other substances of a similar constitution.

Rice, Indian corn, beans, peas, linseed, potatoes, beet are used in husbandry in large quantity with great effect for fattening, that is, for the increase of flesh and fat. In Bavaria beer is used as a stimulating food for the increase of fat.

Whether much or little importance may be ascribed to the universal experience of husbandry, it is certain that animals which are fed upon these different substances, under certain conditions (abundance of food, little exercise, high temperature, &c.), after some time become much fatter than before. This fat proceeds from the food. Rice, beans and peas have been carefully analysed by various chemists. Braconnot found in Carolina rice 0·13 per cent. of oil, in Piedmont rice 0·25 per cent.; Vogel found in rice 1·05 per cent.

According to these analyses, the organism received from 1000 lbs. of Carolina rice 1·3 lb. or 2·5 lbs., or according to Vogel 10½ lbs. of fat.

Peas contain, according to Braconnot, 1·20 of a substance soluble in æther, which he calls leafgreen (chlorophyll). The

bean of the *Phaseolus vulgaris*, according to the same chemist, contains 0·70 of fat soluble in æther. Fresenius obtained from peas 2·1 per cent. of a substance soluble in æther, from linseed 1·3 per cent.

For every 1000 lbs. of peas or beans the organism receives, according to Braconnot 12 lbs., according to Fresenius 21 lbs. of fat, and from as many beans only 7 lbs. of fat.

Beer, as far as I am aware of, contains no fat: Fresenius obtained from the pulp of the beet-root 0·67 per cent. of a substance soluble in æther.

According to further direct examinations made in our laboratory, 1000 parts of dried potatoes gave 3·05 parts of a substance soluble in æther. This substance possessed all the properties of resin or wax; we will, however, assume that potatoes contain $\frac{3}{1000}$ of their weight of fat. Three one-year-old pigs, fattened with 1000 lbs. peas and 6825 lbs. potatoes fresh boiled, which are equal to 1638 lbs. of dried potatoes, increased in weight in thirteen weeks from 80 lbs. to 90 lbs. each. A fully fattened pig averages in weight from 160 lbs. to 170 lbs., and after killing the fat weighs from 50 lbs. to 55 lbs. The three pigs have consumed 21 lbs. of fat, contained in the 1000 lbs. peas, and 6 lbs. in the 1638 lbs. of potatoes, together therefore 27 lbs. Their bodies, however, contained from 150 lbs. to 165 lbs. of fat. There is an increase of from 123 lbs. to 135 lbs. more fat than the food contained. A pig one year old weighs from 75 lbs. to 80 lbs.; suppose it to contain 18 lbs. of fat, there still remains, leaving entirely out of question the matters soluble in æther contained in the excrements, 69 lbs. to 74 lbs. of fat*; the production of which in the organization cannot be doubted, and whose formation remains to be accounted for.

M. Boussingault's examinations concerning the influence of food on the quantity and composition of the milk of the cow, furnish other more important grounds for the opinion that animals produce fat out of certain food, which is neither

* M. Vogt, a butcher at Giessen, in answer to some questions of mine, gave me the following as the result of his experience, which has been confirmed by other intelligent persons:—A restless pig is not adapted for fattening, and however great the supply of food it will not grow fat. Pigs which are fit for fattening must be of a quiet nature; after eating they must sleep, and after sleeping must be ready to eat again. When a pig is a year old it weighs from 75 lbs. to 80 lbs., and if the fat is intended to be used as lard, it must be fed daily for thirteen weeks with 20 lbs. to 25 lbs. of boiled potatoes, and about a measure of peas (2 litr.); towards the end of the time the food may be somewhat diminished. A pig so fattened weighs from 160 lbs. to 170 lbs., and contains of fat and lard, taken altogether, from 50 lbs. to 55 lbs. A pig of a year old has a lard membrane under which the lard is secreted, but at that age it does not contain lard.

fat itself, nor contains fat (*Annales de Chim. et de Phys.* t. lxxi. p. 65).

M. Boussingault's experiments correspond with universal experience, and I believe are to be relied upon; it is, therefore, the more inconceivable to me that he has placed himself by the side of those who support the opposite opinion.

A cow was fed at Bechelbrunn during eleven days upon daily rations of 38 kilogrammes of potatoes, and therefore in eleven days upon 418 kil. Also 3·75 kil. chopped straw; in eleven days, 41·25 kil. In these eleven days she gave 54·61 litres of milk, which contained 2284 gram. butter. As 418 kil. of fresh potatoes are equal to 96·97 kil. of dry potatoes (potatoes contain, according to M. Boussingault, 76·8 water and 23·2 solid matter, *Annales de Chim. et de Phys.* 1838. p. 408); further, as 1000 gram. potatoes contain only 3·05 gram. of soluble matter, and the straw, according to experiments made here, contains only 0·832 per cent. of a substance soluble in æther (a crystalline wax), the cow had, therefore, in eleven days consumed 291 + 343 gram. = 634 gram. of substance, soluble in æther. There was contained in this milk however 2284 gram. of fat.

In another case, in a trial carried on in winter, the daily rations of the cow was for a long time 15 kil. of potatoes and 7½ kil. of hay. The quantity of milk amounted in six days to 64·92 litres. These 64·92 litres of milk contained 3116 gram. of butter. In six days the cow consumed 90 kil. of fresh potatoes, equal to 19·88 of dried; in the same time 45 kil. of hay were consumed. Suppose that the 19·88 kil. of potatoes supplied to the cow contained 60 gram. of fat, the other 3056 gram. of butter must have originated from the 45 kil. of hay. According to this, hay must contain nearly 7 per cent. of fat. This is easily ascertained by experiment.

From hay of the best quality, in the state in which it is consumed by the cows, 1·56 per cent. of a substance soluble in æther was obtained in the Giessen laboratory. Taking the hay to contain 1·56 per cent. of butter, the 45 kil. of hay could supply the cow with only 691 gram., there remains, therefore, to discover whence the other 2365 gram. of butter originated which M. Boussingault found in the milk.

In a note which M. Dumas has appended to a communication of M. Romanet's (*Comptes Rendus de l'Acad. des Sciences*, 24 Oct.), the following remarks are made:—

"Hay contains in the state in which it is consumed by the cow, nearly 2 per cent. of fatty matter. We will show (MM. Dumas and Payen) that the ox which is fattened and the milch cow furnish a *smaller* quantity of fatty material than

the fodder contains. As regards the milch cow in particular, the butter in the milk corresponds very nearly with the quantity of fatty material contained in its food; at least as far as in that of the food we have yet studied, namely hay and Indian corn, which last the cow does not usually obtain as food."

After the foregoing facts, which I could considerably multiply, it will be very difficult for MM. Dumas and Payen to prove that the cow, for instance, furnishes from the fatty matter contained in the food only the corresponding quantity of butter. The proof of the supposition, besides, that animals receive the fat in their food in the same state as it is found in their bodies, is impossible. Nothing is easier to decide than the question whether or not the butter which the cow produces, is contained as butter in the hay.

Hay gives after exhaustion by æther a green solution, and on evaporation a green residue, with a strong agreeable smell of hay, which possesses no properties characteristic of fatty substances. This green residue consists of various substances, of which one is of a waxy or resinous nature, known under the name of chlorophyll; another ingredient of the same crystallises from a concentrated æthereal solution in minute laminæ, and is the crystalline wax which Proust obtained from plums and cherries, from the leaves of cabbages, from a species of *Iris*, and from grasses, and which is probably identical with the wax that Avequin collected in such large quantities from the leaves of the sugar-cane. M. Dumas has analysed this substance, and found it to differ both in composition and properties from any of the known fats; in consequence of which he felt justified in giving the name cerosine to this substance.

M. Fresenius obtained by means of æther from straw, and M. Jägle, of Strasburg, from the fresh plant, *Fumaria officinalis* (in the Giessen laboratory), by means of alcohol, a crystalline wax, very similar to cerosine. The occurrence of wax in the vegetable kingdom is very extensive, generally accompanied by chlorophyll.

Margaric or stearic acid, the principal ingredient of the fat of animals, is neither found in the seeds of corn, nor in herbs nor in roots which serve as food. It is evident that if the ingredients of the food soluble in æther are convertible into fat, margarine and stearine must be formed out of wax or chlorophyll.

As far as our experience goes, the chlorophyll of the food taken in a green state is given out from the body unchanged; even in man the excrements retain the colour of the green vegetables consumed. It is also considered that the wax does

not experience any change in the organism. All doubt may be removed by the simplest experiments; it may be shown that the excrements of the cow contain as much of the substances soluble in æther as has been consumed in the food. The excrements of a cow which was fed upon potatoes and grass were dried and exhausted by æther; a green solution was obtained, somewhat darker in colour than that given by hay, which upon concentration owed its consistency to a white crystalline waxy substance, which was surrounded by a dark green mother liquor. Upon further evaporation it gave out an unpleasant smell, and left when dried at 100° C., 3·119 per cent. of the weight of the excrements of fat and similar substances.

As M. Boussingault has found that the dried solid excrements (*Annal. de Chim. et de Phys.* t. lxxi. p. 322) amount to four-tenths of the weight of the dried fodder, it is evident that these excrements contain very nearly the same quantity of fatty substances as the food consumed.

7½ kilogr. of hay contain (at 1·56 per cent.) 116 gram. of fat. The 15 kilogr. of potatoes contain further 10 gram. of fat. In the whole, therefore, 126 gram. of fat.

The solid daily excrements weigh 4000 gram.; they contain (at 3·119 per cent.) 124·76 gram. of fat. A cow which produces in six days 3116 gram. of butter, consumes in its food during the same period 756 gram. of substances soluble in æther, and gives off in her excrements 747·56 gram. of substances of the same nature and properties; it must therefore follow, that in the production of 6¼ lbs. of butter in the milk, these ingredients of the food can have no share.

I consider I have now demonstrated that the fat which accumulates in the bodies of animals during the fattening process, and that the butter daily produced in the milk, do not originate from the wax or chlorophyll of the food, but from the other ingredients of it. I think I should be giving myself unnecessary trouble to look after facts to correct M. Dumas's peculiar opinion, because upon further consideration it is of a nature to correct itself.

It is similar to the idea of M. Payen, that the oil of potato spirit (fusel oil) is ready formed and contained in potatoes. But since it has been found that the last syrup arising from the preparation of beet-root sugar produces in the distillation of brandy a considerable quantity of fusel oil, no one will doubt its formation during the process of fermentation.

The opinion of M. Dumas is a necessary consequence of the exclusive hypothesis, that animals produce in their organism no substances serving as food (note quoted above),

but that they receive all sustenance, whether sugar, starch, or fat, from the vegetable kingdom.

I agree perfectly in opinion with M. Dumas in relation to the substances which serve for the formation of blood; but differ from him in considering it as fully proved, as far as observation extends, that wax is formed in the body of the bee, and fat in the stall-fed cow.

In regard to the principle of M. Dumas, that the organism of an animal is not able to produce any substance serving as food, it is equivalent to saying that the organism produces nothing, but only transforms it; that no combination takes place in its body, when the materials are not present by means of which the metamorphosis originates. Thus the formation of sugar of milk in the bodies of carnivorous animals cannot take place, for dog's milk, according to Simon, contains no sugar of milk. Thus also fat cannot be produced in their organism; because, besides fat, they do not consume any non-nitrogenous food. But starch, gum and sugar contain, even with their large quantity of oxygen, all the ingredients of fatty bodies; and the formation of butter in the body of the cow, and of wax in that of the bee, leave hardly any doubt that sugar, starch, gum, or pectine, furnish the carbon for the *formation* of the butter or of the wax.

It is further certain that the brain (Fremy), the nerves, the blood (Lecanu), the fæces, and the yellow of the egg (Chevreul), contain a substance in considerable quantity with a far smaller proportion of oxygen than the known fatty acids, a substance which hitherto has not been found in the food of graminivorous animals. The formation of cholesterine from fat cannot be supposed without a separation of oxygen or of carbonic acid and water; it must be derived from a substance far richer in oxygen in consequence of a process of decomposition or metamorphosis, which, applied to the case of starch or sugar, explains their conversion into fat in the simplest manner.

In the before-mentioned note to the observations of M. Romanet, M. Dumas attempts, from the facts quoted in the preface to my Pathology, to weaken the conclusion to which I had arrived concerning the formation of fat in the animal body. These facts concern the quantity of fat in a goose fed upon Indian corn (maize), which corn I have alleged not to contain a thousandth part of fat or fatty substance. The experiments of M. Liebig, says M. Donné in the *Journal des Débats*, are throughout inexact and false, as MM. Dumas and Boussingault have obtained 9 per cent. of a yellow oil from Indian corn, which M. Dumas had the honour to exhibit to the Academy.

It must be evident to every unprejudiced person, that the fact mentioned in the preface has no necessary connexion with the discussion, concerning the production of fat, in the work itself, or in the appendix; it is not employed in the argument. While writing the preface, a friend of mine communicated to me the result of fattening geese with Indian corn. I found in the *Jour. de Chim. Médicale*, i. p. 353, an analysis of maize by Lespes, in which no trace of a fatty substance is mentioned. I further found by an examination by Gorham, in the 'Quarterly Journ. of Science,' xi. p. 205, that maize contained a particular substance, which he called zein, which was extracted by alcohol and could not be fat, as, on the authority of Gorham, this zein was not miscible with fat oils. Gorham does not mention any fat oil.

Therefore, according to every fact of which I was aware, maize contained neither fat nor any substance similar to fat. I had not myself at that time entered into any examination of it. The results obtained by MM. Dumas and Payen induced me, however, to undertake an examination of Indian corn, which was grown in my garden.

67 gram. of maize were exhausted by æther. The æther left behind, on evaporation on the water bath, 2·849 gr. of a thick yellow oil.

The weight of this oil amounted to 4·25 per cent. of the seed. The difference in this experiment from that of MM. Dumas and Payen is very great; 9 per cent. is so much that this seed might be used with advantage in the manufacture of oil. I consequently altered the mode of examination by a proceeding which insured a perfect extraction. The seeds were treated with dilute sulphuric acid kept at nearly a boiling heat until they had almost disappeared. The residue was washed, dried and exhausted by æther. 77 grm. produced in this manner 3·594 grm. of a substance soluble in æther. Maize grown in the fruitful fields of Giessen, therefore, does not contain more than 4·67 per cent. I found since also an analysis by Bizio (*Brugnatelli Giornale*, t. xv. pp. 127, 180) which gives for Italian maize 1·475 per cent of fat oil.

Maize belongs to those seeds which produce a decidedly favourable influence on the formation of fat; some maize contains no fat (Lespes, Gorham), some contains above 4 per cent. of oil, and other maize contains 9 per cent. of fatty oil. According to each individual's view, arguments may be drawn from these observations favourable or unfavourable to the formation of fat in the animal body; but as the analysis of the excrements of the geese was not made, they cannot be taken into account.

The fatty oil obtained in the Giessen laboratory from the seeds of the maize, completely dissolved in an alkaline carbonate and formed a perfect soap; it consisted of a fatty acid, which probably is formed by the influence of the air on the fat contained in the seed on its becoming rancid.

According to the analysis of Dr. Fresenius, this oil consists in 100 parts of—

Carbon	79·68
Hydrogen	11·53
Oxygen	8·79

and possesses, therefore, a composition similar to known fats.

I consider it certain, that the fat which animals take in their food contributes to increase the quantity of fat in their bodies. We have of this certain and decided proof, in the pathological treatment of persons who daily take a considerable quantity of cod-liver oil.

I further consider it probable that oily fat may pass into crystallized fatty acids; and Wöhler's observation, that fusel oil from corn contains a considerable quantity of margaric acid, finds a satisfactory solution by the experiments of M. Mulder, which make the conversion of œnanthic acid into margaric acid probable.

In the Giessen laboratory the observation was made some years ago, that the oleic acid, in the state in which it is obtained from stearic acid manufactories, produces upon rapid distillation more than the half of a fluid product which on cooling becomes as hard as tallow, and upon expression produces 35 per cent. of margaric acid.

These experiments, which are well worth a closer investigation, render it not improbable that hard tallow might be formed out of liquid crystallizable oil.

Whether similar processes take place in animals, in relation to the formation of many of their compounds, to those that take place in plants, is hardly to be doubted.

The observation by Wöhler of the giving out of oxygen by the infusoria, which led him to put the question, whether the nourishment of these creatures was not dependent upon a similar decomposing process to that of plants, might by accurate examination be soon brought to a decision.

VIII. *Observations on the latest Geological Changes in the South of Scotland. By* Mr. William Kemp *.

THERE is certainly no department in geological investigation less understood, while at the same time there is none that has been more frequently treated of, than the last bene-

* Communicated by the Author.

ficent and beautiful re-modeling of the earth's surface, by which it was adapted for the habitation of man. All see and acknowledge that some powerful agency has been brought to bear upon it over its whole extent (during the æra) while it was slowly emerging out of the bosom of the troubled deep; but various phænomena have been pointed to which the tidal wave and rapid currents could never accomplish. Floating icebergs and glaciers have of late been likewise alluded to by master minds of the science, which are now hailed by many, and as observation progresses, will throw much light upon certain phænomena which formerly appeared so dark and dubious. It is with the greatest diffidence that the writer presumes to give his opinion upon such an abstruse subject, but the field is open to all; and as the faculty of comparing and judging of cause and effect is not confined to the great and learned alone, obscure individuals have sometimes given hints which have led to splendid results, and he submits the following observations to the public from no other motive than an anxious wish for the elucidation of truth. He is aware that such a subject will attract little notice, unless introduced by some great familiar name; however, the pleasure arising from years of patient investigation has been a rich reward, let its reception be what it may.

Perhaps there is no part of Great Britain where the later changes upon the earth's surface can be studied to greater advantage than the district around Galashiels. Upon every hand we have vast accumulations of boulder clay flanking the hills, together with beds of gravel and sand overlying the lower declivities, besides numerous examples of what is called *crag* and *tail*, profusely strewed over with erratic boulders radiating fan-like towards the east; we have likewise broad and well-defined terraces high up the hill-sides, which in themselves are objects of great interest; and lastly, there are the hitherto unaccountable, tortuous, angular ridges of gravel parallel with, or partly stretching across the valleys. All these taken together combine to give evidence well worth the attention of the profoundest intellect, as so many written characters of a past æra of the world's history traced by the hand of that all-pervading Power, whom alone all the laws of nature obey. Placed in such a favourable locality for observation, the writer has had his mind strongly impressed from time to time, as he followed up the investigation of the varied and striking appearances around him. His judgement may be at fault respecting the cause of some of those appearances, but at all events he would disdain to give willingly a woven tissue of theory unbased upon facts; his conclusions, therefore,

are such as naturally arose from oft-repeated visits to the various localities, and from a careful examination of their position, formation, and general and particular features. Many hasty ideas had to be rejected as subsequent investigation proved them to be erroneous, and consulting writers was ofttimes rather a stumbling-block than a furtherance. However, several writers of late seem clearly to have pointed towards the truth, though they commonly appear to impute too much to any one cause. The following are the deductions which the writer feels at present warranted to make from his own personal investigations.

Previous to the emerging of this island out of the bosom of the ocean, of course the greater portion of its surface would be bare rock, most likely strewed over with a considerable accumulation of stony debris; and whatever may have been the cause of the denudation, or scooping out of the lower valleys, it is evident that it must have been going on previous to, or at the time the higher hills were emerging above the surface of the water, for it is clearly evinced by the fact that the subsequent debris of the hills rests upon these lower tracks. Long before the emerging of the land, volcanic action had been very prevalent, as is shown by the numerous ridges and conical hills of trap, which have all been thrown up under a deep sea; indeed there is no evidence of any one in all Britain having burst forth in the open air. But after these submarine eruptions had ceased, that mighty internal power which occasioned them was still in existence, and as its expansive force was no longer relieved by bursting through the surface, it seems to have acted in another manner and elevated simultaneously the whole island. However, from various appearances we are enabled to conclude, that this elevation was not accomplished by a few overwhelming convulsions, but by slow degrees through the lapse of ages; and indeed for anything we can know it may be still slowly progressing.

The most striking peculiarity, and one which is very obvious to all observers, is that of oceanic currents having swept the detritus of the rising heights from west to east, so that it is universally seen flanking the eastern declivities of the hills, distinguished by that peculiarity of form known by the designation of *crag* and *tail*. Such peculiarly formed accumulations are spread out in many places to a great extent and thickness; we have examined places where water has worn it down for about 100 feet deep in ravines, and along the sides of the valleys. It is the opinion of some eminent geologists that this boulder clay is of various ages, arising from a difference in the appearance of the mass; to this we give assent so far, that

is, as to ages, but not as to different epochs of geological time; for as we have before stated, the scouring out of the valleys had taken place previous to the deposition of the boulder clay, as is clearly shown by the latter being less or more spread over the former. But there is another consideration which we cannot overlook; the detritus seems all to have been driven in the same direction, and locally the boulders are mostly all of the same material. Again, it is so frequently interspersed with such huge masses of rock as are never found in any of the older strata, which gives strong ground for believing that those extraordinary masses have been struck off the prominent rock, and borne to a distance at a comparatively recent æra, by such a combination of powerful agents as more primæval time does not exhibit. At some places there is a well-marked distinction in the mass, where the lower beds differ in colour, and are more or less argillaceous than the superior; the lower, likewise, rises with a higher inclination towards the hills, while the incumbent beds are generally of a lighter colour, and more arenaceous in composition. We can frequently detect small boulders of foreign rock, such as sandstone, &c. of the coal districts, and various rolled fragments of the trap family; the first must have travelled a great distance from the west, or north-west, while the latter may belong to the numerous trap dykes which intersect the district. These are chiefly to be found along the valleys, but seldom upon the steep escarpments of the hills. We commonly observe that boulders of a large size are not so plentifully interspersed in the clay, where it has evidently been driven to a considerable distance, and where they do occur they do not appear to have been rolled. At many places we can scarcely pick out a boulder exceeding two or three lbs. weight, the deposit being a homogeneous mass of clay and small pebbles: in other places, where the debris has been driven to a considerable distance, the boulders are exceedingly well-rounded, while the opposite is the case with that resting upon the escarpments of the hills, where fragments of all sizes are seen indiscriminately mixed, with scarcely a rounded angle.

But in order to understand these remarks, it will be necessary to examine the appearance of the hills, and describe their various features downwards. It is not requisite to point to the denuded summit of the hills with respect to this part of the subject, for that those summits have been much denuded is what none can deny, it being so striking and obvious to all. While the denudation was going on, the grosser debris would be forcibly driven over and rolled down the sheltered side of the declivities, and the finer water-borne

matter would be carried to a greater or less distance, according to its specific gravity; so while the rocky fragments were being deposited upon the flanks of the adjacent heights, the finer sedimentary ingredients borne along from the more distant peaks would gradually subside and be deposited along with the grosser fragments; and in this manner would the coarse and fine become indiscriminately mixed, as we find them. As the hills arose and their higher summits had become elevated above the action of the sweeping water, the currents would take different directions from their former onward course, removing a part of the earlier wreck, which, together with the spoil still derived from the hills, would be laid over the more distant parts of the former deposit, containing many boulders more rounded and smoothed by attrition. At last, when the land became so far elevated that only partial currents swept through the lower straits, there would in many places be a still further remodeling, for the finer particles would be swept into sheltered localities according to circumstances, while the fragmentary debris would be rolled along and thrown up in banks of gravel.

From the same denuding and sweeping cause do we account for the greater part of those large boulders, which are so plentifully scattered over the surface along the declivities to the east of the eminences, where in many moorish districts they lie yet unremoved. East from the village of Fans they may be counted in thousands; and so thick do they lie upon the surface, that a person may almost walk along continuously from one to another. They are of all sizes, from a few pounds to several tons in weight; the greater part are rather well-rounded, but that does not argue against the above theory, as there can be little doubt that many of them would be well smoothed over upon the one end before they were torn from the living rock, and upon examination many of them appear to have been so. Fans occupies a rather elevated situation, being built upon a broad flat knoll of hard crystalline greenstone. A quarry at the west end of the village opens up a fine view of the rock, which is of a beautiful columnar structure. The columns are about 30 inches in diameter, standing nearly perpendicular, and although closely joined can be easily separated, which shows that its denudation would be much more easily accomplished than that of a rock of a more massive structure; and hence the vast number of these blocks radiating towards the east of that place. A question may arise, how comes it that those boulders are so thickly scattered over the surface—not imbedded in the clay, but lying upon it? That question has already been partly answered. As the rock

wore down, the fragmentary deposit behind becoming exposed to the current would be partly carried away, rolling the boulders further along, and finally leaving them upon the surface. At the same time it must be understood that they are not confined to the surface alone, as they are plentifully found at various depths.

Such is that part of the phænomena in question, which is the most obvious, and which has been so frequently treated of by various writers; but there are other appearances yet to be noticed of an extremely interesting character, which, taken in connection with the foregoing, seem to shed a ray of light upon those long past revolutions of ancient time. In order to point to those we must again ascend the hills, and draw attention to the deeply engraved characters upon their rocky shoulders. We here in the first place allude to the terraces, which in many places are so broad and well-defined upon the hills in the district around Galashiels*. The writer has carefully taken the level of those various shelves from hill to hill, across valleys, &c., over a wide district, and has ascertained beyond a doubt that they are correctly level or parallel in elevation throughout.

The cause is very obvious why those terraces have remained so long undiscovered; their situation is very different from those celebrated ones of Glen Roy; at the latter place they are visible to the spectator for many miles along the steep grassy banks of that Highland glen. But here no such appearance is presented to the eye, as we have no continued range of hills; the highest are only a few insulated peaks, overtopping a number of rounded and irregular undulating hills; it is only upon some of the sides and projecting shoulders of these that those terraces are to be seen. He that would wish to survey them, requires to provide himself with a proper levelling instrument, and to travel patiently from hill to hill, and then, but only then, will he be satisfied with the truth of our assertions: we proudly appeal to those faithful and enduring witnesses, whether for or against us.

These terraces are not a single range but a series, extending from the summit of the hill downward; they average 54 feet in perpendicular height one above another, some less and others more. We assume each of those to have been successively the level of the ocean for an indefinite period of time. We do not mean to state that the land had been raised 54 feet at once by any sudden movement; but that during its eleva-

* For a more particular account of those terraces, see Chambers's Edinburgh Journal, No. 444.

tion it had been stationary at those levels for a longer period than while it was emerging through the less-worn spaces between. It is a remarkable feature in those terraces, that they can scarcely be traced but upon the north and south projecting shoulders of the hills, such as have been most exposed to the sweeping currents from the west. The most distinguished of those terraces is one upwards of 800 feet above the level of the sea; as a great number of our lesser hills considerably exceed that height, it might be expected to be pretty generally marked; and not only is it so, but in a very remarkable degree. According to the situation of the hills, we have traced it from where it was merely visible to where it was 300 feet broad; in many places it exceeds 100 feet, and everywhere it seems chiefly scooped out of the solid rock. As we ascend to the superior levels, the traces of each terrace become fewer and more distant as they overtop the hills; still upon some higher eminences they are very well defined, as for instance where they are so remarkable upon the north side of the Eildon hills. From the first-mentioned terrace downwards they become gradually less and less distinct, which is a further confirmation of the theory in question, because the abrading action would become gradually less powerful as the land towards the west arose and checked the current. Owing to the detached and rounded form of the hills, those shelves are nowhere of any great length; few of them exceed 300 yards, and many are not so much. However, upon Ruberslaw, a high conical hill about 6 miles west from the town of Jedburgh, there are two terraces upwards of 800 paces in length by 30 in breadth, and another 600 still broader. These are very beautiful, and in some respects they are the finest in the district. To the east is the valley of the Rule, and on the other side the ground rises to a high ridge, extending eastward to the town of Jedburgh. Near the north summit of this ridge, a finely marked terrace runs along its whole length, which is about 1½ mile, and which correctly corresponds with one of those upon Ruberslaw; indeed so truly does it agree with the instrument, that we can detect no deviation along its whole course.

Let us conceive in the mind's eye an immense body of rushing water sweeping along like a mighty river, would that be sufficient to account for those terraces? certainly not. We cannot suppose that flowing water alone would run out those shelves, as it would scour indiscriminately the surface over which it swept, or rather it would act more powerfully upon the lower depths by the superincumbent pressure of the water. Then let us suppose that the sea was comparatively tranquil, just as it is at present, being occasionally raised into fury by

the driving blast: on this supposition, the lashing of the stormy waves together with the tidal action would certainly in the course of time excavate a beach of less or greater magnitude, according to the nature of the ground and its exposed situation. But a close observer must reject that idea also, as he will at once perceive that powerful currents must have swept along, which is incontestably proved by the vast mass of ruins so universally thrown to the east, enough in many cases to afford material for a city. During our first examination of the terraces, we were frequently puzzled by a singular appearance they presented. In almost every case, whether those shelves were of greater or less extent, their extremities were always observed to be rounded over, especially at their western end, and whenever the ground trended back, they still kept a nearly straight course by bending down hill for some distance. From this mysterious form and other inexplicable appearances, we were, after repeated examinations, at last obliged to abandon the idea of water alone having run out those terraces. Early in the spring of 1841, as the writer was wandering upon one of the finest scenes of the kind in the district, it then occurred to him, that probably floating icebergs was the cause of the extraordinary denudation around him; gradually the mystery seemed to vanish, and shortly after he became so thoroughly convinced of the truth of the theory, as to be surprised at his obtuseness of intellect in not having caught the idea earlier, so plainly did that place seem to tell its own tale. The scene alluded to is upon a saddle-backed ridge or spur of Williamlaw hill, which stretches its denuded spine and terraced front boldly south into the Gala valley. The grooving over the summit of that ridge is the most remarkable of any we have seen; at one place the rock is worn down about 10 feet below the adjoining strata, and 36 feet broad. The rock everywhere bears strong marks of attrition. The grooving runs in the direction of the tilted strata, which is nearly east and west. The west side descends at an angle of 26 degrees, and the grooving continues downwards for about 76 feet, gradually diminishing in depth as it descends. The east side has much the same appearance, but is nothing like so strongly marked. Some adjoining ledges of great hardness project several feet high, completely rounded over. The rock is greywacke, the strata nearly vertical, and harder than many kinds of granite*. That place is by no means a solitary in-

* In the face of this evidence, there are not wanting some who assert that the rounded form of those prominent blocks has been occasioned by weathering, "for (they say) had the place been subjected to the powerful denudation of floating icebergs, the surface would have been as smooth as a

stance of the kind; it is certainly unequalled in this district for showing much in little space; however, we could point to many places having the same characteristics, and many of them scarce inferior in appearance; and as all the terraces are rounded over in the same manner at their extremities, we may infer the cause to have been the same. Moreover, all these are highly elevated and exposed to the open west, where floating icebergs borne along by a tumultuous sea would strike off and abrade the rock exposed to their action with almost irresistible force; and as masses of considerable size sail deep in the water, their bottom would first strike the ground, and be driven over the still sunken ridge with vast increased pressure*. Such a process going onward for an unknown period of time, for a few months in each succeeding year, seems clearly to account for these phænomena, and we should think that the most sceptical would concede to it upon inspection.

There is another division of the subject which has attracted much attention, and one which we think cannot be solved without the aid of floating icebergs. We now allude to those large angular masses of stone which are so frequently found in situations far from the parent rock, and which differ very much in appearance from such rounded boulders as have been rolled along the declivity behind the height they were torn

well-polished flagstone, and all the protuberant blocks would have been dressed down to an even surface:" as proof, they allude to such polished surfaces upon the Swiss Alps. In answer to the above objections, in the first place we must admit of weathering to a certain degree; however, it is known that the hard blue rock in this district almost defies the penetrating tooth of time. There are many old towers in the neighbourhood built with that stone, whose aged walls have withstood the vicissitude of the seasons for many ages, and where the edges of the stones are as sharp, and the dint of the hammer as legible as if they had been erected recently. Besides, we have examined rocks that had been previously covered with a coating of debris, which presented the same rounded appearance. In the second place, why compare this rock with granite? Granite being an unstratified rock, consequently if not upon a steep precipice, will almost resist any conceivable power to tear it away, except the mechanical wearing down of the surface; hence its polished appearance. But very different is the case with the broken edge of the stratified greywacke, where in this district few of the beds exceed 18 inches in thickness; besides, the beds are crossed in all directions by fissures (joints), so that the larger blocks may resist denudation for a time; at last they are borne away, but a hollow is left in their place, while the next in height becomes prominent until it yields in its turn, and so on continually.

* We were much pleased to see the same idea taken by that able geologist, Mr. Maclaren. Describing the striated rocks of Corstorphine hill, he says, "An iceberg, for instance, deep enough to scratch the lower part of the slope, and forced by a current over the higher level, must have been partly lifted out of the water, and its pressure here would be enormously augmented."—*Scotsman Newspaper*, June 25, 1842.

from. Of course they have all taken the same direction, but those alluded to retain angles so sharp as to forbid all idea of their having been rolled. Besides, such are frequently perched upon situations quite adverse to the rolling theory: we sometimes meet with such amongst the round boulders upon the surface, and not unfrequently in the boulder clay, so very flat and angular, that they must have been borne there by a very different conveyance from the others. We need not dwell upon this part of the subject which has been so frequently treated upon by far abler writers, we shall therefore pass on with pointing to one remarkable instance. There is a large angular fragment of green-stone seemingly upwards of ten tons in weight, close by the side of the old road to Jedburgh, and about a mile south from the Teviot. Upon comparing specimens we find that it must have come from Ruberslaw, a high hill about seven miles direct west. The deep valley of the Rule intervenes, besides a considerable extent of rising ground, so that the stone must have been floated over and dropt upon the spot it now occupies. It is in two pieces which are separate a few inches: that fracture possibly took place when it fell upon the ground; certainly it has not been broken recently, and not likely ever by the hand of man.

We now come to the last, but certainly not the least, interesting feature in the district, that is, the mounds or moraines of gravel which from time to time have elicited so much speculation, but which have until of late as it were mocked all attempts to account for their formation. The honour of having first interpreted their true character is due to M. Agassiz, the celebrated Swiss philosopher, whose experienced eye soon detected them upon his memorable visit to this country. Those mounds stand out in bold relief, often in a tortuous steep ridge-like form, which, together with the local situations where we find them, at once testify that they have been thrown up by a very different cause from any which have yet been alluded to. They are totally distinct from the debris which have been swept into the rear of the hills by the combined action of water currents and floating icebergs. The latter is commonly a broad undulating mass, sloping to the east of the rocky heights; or behind a conical hill it takes the form of a flattish rounded ridge, denominated the *tail* of the crag, which is often flanked with gravel in low swelling undulations. But those mounds now under consideration are frequently as narrow, high and steep as the loose material composing them will admit of. We cannot suppose water to have thrown up those mounds into such a sharp ridge, so equal in breadth, so tortuous in their course, and of such a length as some of them are. More-

over, they frequently extend across valleys where currents of water had formerly swept along. There are some very conspicuous moraines to be seen in this district; in the valleys of the Tweed, the Teviot, the Ettrick, and the Gala, &c. As examples of such we point to those very fine ones near Gala House, and the Fairy knolls by the Allen water. A very remarkable one extends partly across the valley a little below the town of Galashiels; it exceeds 140 feet in height by 600 feet in length, extending from the north bank at a right angle across the valley: that is only its remains, for it is evident that it had once crossed from side to side, for opposite upon the top of a high bank a portion of it is still very prominent. However, as it would form a barrier to the Gala, it has subsequently carried a great part of it away. The turnpike road passes over the north end of this mound, and as its steepness there has long been a cause of complaint, last summer workmen were engaged in excavating the height, and have opened up a highly interesting section about 12 feet deep where it bends to the west, adjoining what may be termed the lateral moraine. At the east side, below a mass of gravel and sand, there is exposed a large quantity of rolled stones each from about 5 to 13 pounds weight, which appear as if they had been tumbled together without any admixture of smaller material, so that we may thrust in our hand between the boulders. These are seen along the lower edge of the cut about 14 feet, and 3 feet high,—how deep we cannot say. The bank above those stones, although having visibly a stratified appearance of small and coarse gravel alternating with intervening portions of boulder clay, is yet so strangely contorted, especially a little further west, where the thin beds of fine gravel become quite vertical at more than one place, as to confound all idea of its having been finally laid there by aqueous deposition. In fact we have therein displayed the formation of that mound in characters infinitely easier to understand than those of the ancient Egyptians. It is well known that a glacier bears a considerable quantity of debris upon its surface, which it grinds off the ground in its course, beside what rolls down upon it from the adjoining bank; in this case we see that a current of water has run along its surface carrying away the lighter material, while the glacier bore along the grosser debris which would be deposited at the extremity. Anon the water has changed its course, while sand, clay and stone were next deposited, and so on alternately in a greater or less degree; and after that semi-stratified mass was laid there, the immense pressure of the glacier had thrust it up in the contorted manner above described.

Perhaps there is not a finer example of those moraines in Britain than that celebrated mound known by the name of the Bed Shiel Kames: that beautiful moraine, taking in all its sudden bendings, is about $2\frac{1}{2}$ miles long; its height is from 15 to 60 feet; besides, a great part of it is buried to an unknown depth in the morass. It runs along the middle of an extensive swamp called Dogton Moss in Berwickshire, about 3 miles north from the village of Greenlaw, and near the south base of the Lammermuir hills, from whence the debris composing it is chiefly derived.

Having lately been informed by a very intelligent gentleman at Rule water, that he supposed he had discovered a fine moraine in the south border of Roxburghshire, in a late tour, in following up other investigations in that district, we made a point of visiting that locality, and were not disappointed. By its striking appearance we soon caught a view of it, although still at a considerable distance, by the aspect of the vegetation which clothed it, which differed so much from the neighbouring hills, or the black heathy moor around it. As we approached the place we found the gentleman's account of it to be exceedingly correct; it is situate upon the lower corner of an upland vale which rises with a considerable acclivity towards a crescent bend in the Carter fell, which is about a mile distant south. Close by the moraine to the north-west and west, the ground rises rather rocky and precipitous to a height of about 40 feet; two small mountain burns join their waters at a short distance behind the moraine, and run down a narrow gorge that the mound must once have choked up; but those streams have subsequently cleared their way and rounded the mound to its present form. However, it is evident that ages have elapsed, and may again roll by, without those tinny rills making any further alteration upon it. That mound is known by the name of the *Scaud-law*; it is of a circular form, about 635 paces in circumference, and 80 feet high. From base to summit it is wholly composed of the rocky debris of the neighbouring hills, which debris is of all sizes, from coarse sand to blocks of many ton weight, consisting of sandstone, shale and lime, confusedly tossed together, lying at all angles, and peering out of the surface like tombstones in a country churchyard.

We have been particular in describing this singular mound, as we deem it one of the strongest evidences of the glacier theory we have witnessed. The drifted debris there as well as elsewhere seems likewise to have been driven to the eastward, but that composing this mound has been carried in a nearly opposite direction, that is, to the north-west. Nor is that a so-

litary exception; for wherever we have observed those mounds, they are always situated according to the natural declivity of the ground in the vicinity of elevated ranges, without regard to any direction. We could direct attention to many more of a similar character, but as we have drawn out this paper to a greater length than was anticipated, we hasten to conclude as briefly as possible with a recapitulation of the principal points.

When the summits of our hills were emerging out of the ocean, strong currents (perhaps periodically), accompanied with numerous floating icebergs, seem to have been furiously driven along, denuding their summits and sweeping the debris to the east, forming the beds of boulder clay commonly denominated *til*; still as the land was upheaving and the higher peaks rising above the denuding action, the onward wave and ice, ever lashing against their western faces, rendering them bare and precipitous, and sweeping round their sides, cutting out the rocky terraces, grinding down and rounding over those saddle-backed ridges as they were raised to near the surface, while occasionally from some overhanging precipice masses of the rock would fall upon the ice, and be borne along with it until its floating raft gave way, or until it was dashed from its seat and dropt upon the bottom. Anon, while the land by internal throws was rising step by step, a part of the older ruins would be removed to a greater distance, and be again lodged over its lower flanks, and while the lighter debris was being swept away, the larger blocks would be further rolled along and finally left upon the surface. And lastly, by shallower currents, part of the debris, where it was exposed to their action, would be further removed, and thrown up in beds of sand and gravel into sheltered situations. In the course of time, when the valleys had become elevated above the ocean, the accumulating snow upon the hills would commence descending down the declivities in the glacier form, to those land straits where the moraines are still to be seen, bearing along with them a part of the debris which they had collected in their course, and finally retreating after their beneficent task was accomplished, leaving those imperishable records to attest that they had once been there.

We understand that the glacier theory is rejected by some, who reason in this manner:—Animal and vegetable life, during the æra of the carboniferous system, was such even in our northern latitude as could only have existed in a climate such as the tropics, and although a progressive change had been going on until the æra of the newer tertiary, still even then it had been much above the present. We hold that argument

to be by no means conclusive, but rather the reverse: during the tertiary formation our island had been submerged in the bosom of the ocean, and as we know not how many ages may have elapsed between that period and its subsequent elevation, may not the temperature in that interval have been reduced to a sufficient degree as to become suitable to the formation of glaciers? Certainly, without seeking proof for either side, which is not yet obtained, we may as readily admit that at the æra in question the temperature had been a few degrees lower than it is at present, as that at the carboniferous epoch it had been so much higher; for any thing we know there may be a cycle of change going onward in the roll of time unknown to man, who seems to be but a creature of yesterday.

But to conclude: do we not see in those later changes a great and grand design? Had the land been elevated with all its hard and serrated rocks unreduced by attrition of a very powerful kind, what may we suppose it to have been but so many piles of rugged rock, ever and anon sending down some loose fragments covering over their flanks with a totally barren and impenetrable crust, rendering the greater part for ever a howling desert? We see that the whole operation has been guided by a mighty mind; the various elements of nature have each been called upon to act their destined part, and well we see they have been performed. What is more beautiful than our finely rounded hills, covered with verdant turf to their very summits, together with the smoothed undulating uplands, and the fertile and smiling valleys, altogether forming a rich and beautiful dwelling for man?

Galashiels, Jan. 25, 1843. WILLIAM KEMP.

IX. *On the Analogy between the Phænomena of the Electric and Nervous Influences.* By MARTYN J. ROBERTS, *Esq., F.R.S. Ed.**

[Continued from vol. xix. p. 38.]

31. IN my last communication on this subject I pointed out the striking analogy that exists between the nervous and electric influence as displayed in the action of electricity upon fluids flowing through capillary tubes,—the corresponding action of nervous influence upon the circulation of the blood through capillary vessels,—the explanation the phænomena of inflammation receives from this view of the subject,—the elucidation of that of turgescence;—and the probable cause of

* Communicated by the Author.

suffusion in the act of blushing was shown when the action was viewed as analogous to or identical with electric phænomena. I now proceed to consider this analogy as displayed in reflex action, premising that at present the heads merely of the theory are given, reserving for a future occasion the development of the subject in its fullest details. Theories are often condemned without a hearing by some who pride themselves upon being mere practical men, to such with all due respect I will quote the words of a learned author: "Where a definitive explanation of phænomena is yet impossible, an hypothesis which is not opposed to the facts, but on the contrary accords with them and which opens a new field for research, is admissible even in an exact science founded upon facts."

32. We may consider reflex action as merely motion excited in muscles by irritation of sensitive or incident nerves in these or adjacent parts, and that sympathetic action is motions referrible to the same cause. All these reflex motions, whether of the sympathetic or other parts of the system, appear to me to bear the closest resemblance to electric induction, and that the current in the sensitive and incident nerves induces an action in the nerves of motion that lie contiguous to them.

33. Suppose A A to be a wire through which a current of electricity can be passed, and that another wire, B B, placed close to the first forms part of a closed circuit that is offering a perfectly continuous path for the transmission of the electric fluid; then if a current be passed through A A in the direction of the arrow, it is found that at the moment of the first passage of the current it creates or "induces" another current in B B, and in a direction contrary to the primary current in A A; but this secondary or induced current is only of momentary duration, no trace of its existence appearing after the first instant of passage during the whole continuance of the primary current; nevertheless the moment we annihilate the current in A A a new current is induced in B B, but in an opposite direction to the first induced current. Such are some of the phænomena caused by the action of a current of electricity upon neighbouring conductors, and such also there can be no doubt is the action of nervous currents upon nerves contiguous to others conveying these currents.

34. It will be seen that one condition necessary to the production of these phænomena in wires by electricity, is that of a perfect continuity for conduction in the channel conveying the electric fluid; for if the wire B B be broken no current can be induced in it; and I think it will be found

That every voluntary motor-nervous filament forms a distinct closed circuit.

That every sensitive filament forms a distinct circuit.

That every incident filament forms a distinct circuit.

That every reflex filament forms of itself a distinct closed circuit.

That every incident filament of the sympathetic system (if I may be allowed the expression), which conveys the impression to its centre, forms in itself a closed circuit; and lastly, that every reflex sympathetic filament in proceeding from its centre forms a closed circuit.

These several nerves are each of them looped at the extremity of their course, and no doubt looped in their centre, such as the spinal cord; and it is highly probable that the mass of the spinal cord is but a congeries of these loops completing the circle, or at all events that the mass of the gray matter closes the circuit of the several nerves leading into it. If a nerve be divided all reflex action ceases, because the conditions necessary to the production of inductive action no longer exist.

35. Another condition necessary to galvanic induction, is contiguity of the channels conveying the primary and the induced currents. It will be found that reflex motion exists only in muscles whose motor nerves are contiguous to those sensitive or incident nerves which conveyed the impression of the irritation that produced the so-called reflex motion. I might venture to assert, that in most instances where reflex action is produced, the nerves conveying the impression and those of motion are bound up in the same sheath; and it is probable that the plexuses in the axillary and inguinal region of animals are for the purpose of adjusting these nerves in their contiguous groups before their distribution to the organs. I have used the term "reflex," it being a word well known as designating this species of involuntary motion; but I would be inclined to prefer the expression of *induced motion* for all that class of nervous action which proceed from the centre to the periphery, excited by currents in the sensitive and incident nerves proceeding from the periphery to the centre.

36. The conditions necessary for inductive action having been shown strictly to exist in nerves that produce involuntary motion, it can also be demonstrated that the peculiar action produced by induction occurs whenever involuntary, reflex or *induced* motion is produced: in Section 33 it has been shown that electrically induced currents are of momentary duration, and of a like kind are all involuntary motions; even tetanus is a rapid series of spasmodic interrupted contractions. We have also illustrations of these interrupted actions in the rhythmic

pulsation of the heart, the action of the alimentary canal, the ducts of glands, and, in short, in most of the muscular contractions of the organic life.

37. When speaking of these as "reflex actions," I do not in using their term follow the theories of either Prochaska or Dr. Marshall Hall*, who have classified these motions under this head: so far from agreeing with Dr. Marshall Hall in considering the spinal cord as the seat of the power of reflex action, I believe it merely closes the circuit of the nerves; and when we find reflex action suspended, from injury to the spinal cord, it is not that the seat of power has been destroyed, but that the closed circuit (as explained in sect. 34) has been broken, and that the spinal cord is not more essential than any other part of the circuit to the production of induced motion.

38. A case of pure induction of one nerve upon another, and this attended with the peculiar phænomena of an induced action produced by the annihilation of the primary current (sect. 33), may be shown by a simple experiment:—Place a piece of zinc upon the tongue and a piece of silver between the upper gums and the upper lip; turn the eyes towards a dark place; then, while the metals are in this position, bring their contiguous extremities together: at the moment of contact a bright momentary flash of light will be perceived; continue the contact, and the sensation of light ceases; but separate the metals, and now a bright flash of light is again perceived at the instant of separation. Here then we have an excitement of the gustatory branch of the fifth pair which induces an action upon the optic nerve, all sensation in which appears to us as light. But it is worthy of observation, that this induced action is only at the first moment of irritation of the gustatory nerve; for although the contact of the metals is continued, and consequently a galvanic current is constantly circulating through this nerve, yet no light is perceived; but separate the metals, we then stop the current of electricity, and at this very moment another flash is seen. What can be more analogous, (may I not say identical?) than these nervous phænomena with the case of electric induction in sect. 33? it cannot be said that this is only an effect of the irritation of the optic nerve by the current of galvanism; for were this the case, the sensation of light would be continuous during the passage of the electric current; whereas we find it apparent only at the moments of making and breaking contact; and beside, the galvanic current is not applied to the optic nerve; indeed, we have a flash when the

* On Dr. M. Hall's theory of the reflex action, see Phil. Mag. S. 3. vol. x. p. 51.—Edit.

current ceases, and therefore it cannot be the irritation caused by it that produces the sensation of light.

39. If light impinge on the iris only and not upon the retina, no contraction of the iris ensues; but if it fall upon the retina, then the iris contracts, even if no light has access to the iris itself; therefore the motions of the iris are governed by sensations traversing the optic nerve, and this sensation induces a current in the motor-nerves of the iris.

40. In the motions of respiration induced by the action of the atmospheric air on the incident nerves of the lungs, in the contraction of the sphincters by the induction of the nerves irritated by the contents of the canal or bladder,—in coughing from irritation of the larynx, in sneezing from irritation of the nose, and in many like instances, we see cases of *induced* motion; and these facts, with those I have before adduced, may at present suffice to point out the perfect analogy, if not identity, between the nervous and electric influences: the more extended details of its application to many other interesting vital phænomena in the healthy and diseased condition of organic bodies will be given hereafter.

London, Jan. 5, 1843.

X. *Notices respecting New Books.*

Lectures on Chemistry, illustrated by 106 *Wood cuts.* By HENRY M. NOAD, Member of the Chemical and Electrical Societies of London; Lecturer on Chemistry; Author of Lectures on Electricity, &c.

WERE it possible we would gladly bestow commendation on any attempt, however humble, to enlarge the boundaries of science; but when books are presented to the public, we must, if we notice them at all, speak of them as we find them. It is with regret that we are obliged to withhold our approbation from the work, the title of which is above given; but we are sure that when we have pointed out its true nature, the author himself will scarcely be surprised at our opinion, and that the public will agree with us that this book has been got up with too great haste or too little knowledge; and in some cases we think it will appear that both these formidable obstacles to a successful undertaking have lent their combined aid.

We cannot afford sufficient space to notice the numerous statements which require correction; we shall, therefore, offer a few observations on the author's History of Chemistry, and then confine ourselves chiefly, if not altogether, to the chapter on oxygen, as affording numerous instances of inaccuracy on a subject of great interest, and yet nowise complicated or difficult.

In the historical sketch, of which the first lecture consists, an account is given of the labours of nearly forty chemists; with all of these but one, discordant as their theological opinions must have

been, our author seems to agree, for Dr. Priestley alone is selected for reprobation on this subject, with which, we may remark, the author had no business to intermeddle. It is, he says, "upon his philosophical writings the reputation of Dr. Priestley must rest; his theological opinions are most deservedly condemned." Does the author thus go out of his way to anathematize Dr. Priestley for heterodoxy, lest his own reputation for orthodoxy* should suffer by having lauded the Doctor's discoveries in science? We cannot imagine the existence of any motive more favourable or more weak. We may remark that no mention is made of the labours of the late and lamented Dr. Henry; and Dr. Wollaston is stated to have died in the 53rd instead of the 63rd year of his age.

We now proceed to consider the chapter on oxygen, and in page 140 the following statements occur: "Every human being on the face of the earth consumes nearly twenty-five cubic feet of oxygen every twenty-four hours (45,000 cubic inches daily, according to Lavoisier, Seguin and Davy), and one hundred weight of charcoal requires for its combustion thirty-two cubic feet, yet notwithstanding this immense hourly consumption, the quantity of this essential principle is not diminished in the atmosphere, but bears the same proportion to the nitrogen the other ingredient, *now*, as it did centuries ago."

We shall not object to the statement that 25 cubic feet of oxygen are in twenty-four hours consumed by every adult, but we may observe that this quantity is equivalent to only 43,200 instead of 45,000 cubic inches. The experiments of Messrs. Allen and Pepys give only 39,354 cubic inches, while according to Liebig's statement, that 14 ounces of carbon are daily discharged from an individual, the oxygen consumed in twenty-four hours will amount to 47,480 cubic inches; we will therefore take Mr. Noad's statement of 25 cubic feet, which would combine with nearly 12·75 ounces of carbon; if then 12·75 ounces of carbon require 25 cubic feet, 112 pounds of charcoal, or 1792 ounces, will combine with *three thousand five hundred and thirteen* cubic feet of oxygen, instead of 32, as stated by our author. It is not easy to imagine how so enormous a blunder could have been perpetrated in the first instance, but it is truly marvellous that it did not occur to the author in correcting his proof, that according to this statement "every human being on the face of the earth" must by respiration give out $\frac{25}{32}$ of 112 pounds of charcoal, or $87\frac{1}{2}$ pounds in every twenty-four hours.

The assertion of Mr. Noad, though probably true, that the quantity of oxygen in the atmosphere is the same that it was centuries ago, is utterly incapable of proof; for oxygen itself not having been discovered 70 years, we presume Mr. Noad will admit that no experiments could be made on its quantity before it was known.

There is much requiring correction in the author's statement respecting the means to be employed for procuring oxygen gas: for example, we are informed that a pound of peroxide of manganese of good

* As he sets up for an authority in theology, it is to be hoped he is freer from errors on the subject than we have found him to be in chemistry.

quality will yield four gallons of oxygen gas, and that this is about "the maximum obtainable quantity." But the author states that 1638 grains of peroxide of manganese yield 200 grains of oxygen gas, consequently 7000 grains, or one pound, will give 854 grains; then as 34·4 grains occupy 100 cubic inches, 854 grains will give 2482 cubic inches, which divided by 277·3, the cubic inches in a gallon will give $8\frac{8}{10}$ gallons of oxygen gas. Of course this is supposing the peroxide of manganese to be pure; but even if the impurity amounts to 50 per cent., the quantity of oxygen will be greater than that stated as the maximum obtainable from peroxide of "good quality."

Mr. Noad also states that the 1438 grains of oxide left after heating 1638 grains of peroxide of manganese is "a mixture of 992 grains of deutoxide and 446 grains of protoxide of manganese." The fact however is that the 1438 grains are red oxide of manganese, which are certainly equivalent to, but are not a mixture of, the two oxides named, for deutoxide cannot exist at the temperature requisite to produce 1438 grains of red oxide from the stated quantity of peroxide.

Several errors occur in the author's statements respecting the production of oxygen gas from chlorate of potash: we are informed that half an ounce (218·75 grains) should yield 270 cubic inches of oxygen gas, he having just before stated 1532 grains yield 600 grains of oxygen, 218·75 therefore give 85·7 grains, measuring only 249 instead of 270 cubic inches.

The following note on this subject at p. 144, exhibits an extraordinary degree of confusion of per-centage, weight and measure:—"I find that the best chlorate of potash yields from 96 to 98 per cent. of pure oxygen." For some time we were puzzled to attach any meaning to this statement, but at length we concluded that the following is what the author meant:—"I find that 100 grains of the best chlorate of potash yield from 96 to 98 cubic inches of pure oxygen gas."

Confusion of a somewhat similar kind, though not quite so glaring, is observable in the following statement: "from an equivalent of the oxide [of mercury] we get 100 grains, or nearly 300 cubic inches of oxygen gas, and 1266 grains of mercury are found in the receiver." It should have been stated that from 1366 *grains, which may be considered as representing an equivalent*: but according to the author's method equivalents are not relative merely, but absolute weights.

We had noted many other statements contained in this work for observation, but the length to which this notice has extended precludes our further proceeding.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxii. p. 490.]

Feb. 23, 1843. THE following papers were read, viz.—

1. "Researches on the Decomposition and Disintegration of Phosphatic Vesical Calculi; and on the introduction of

Chemical decomponents into the living Bladder." By S. Elliott Hoskins, M.D. Communicated by P. M. Roget, M.D., Sec. R.S.

The object of these researches was the discovery of some chemical agent, more energetic in its action on certain varieties of human calculi, and less irritating when ejected into the bladder, than any of the fluids hitherto employed.

These indications not being fulfilled by dilute acids, or other solvents which act by the exertion of single elective affinity, the author investigated the effects of complex affinity in producing decomposition, and consequent disintegration, of vesical calculi.

For this purpose an agent is required, the base of which should unite with the acid of the calculus, whilst the acid of the former should combine and form soluble salts with the base of the latter. The combined acids would thereby be set free in definite proportions, to be neutralized in their nascent state, and removed out of the sphere of action, before any stimulating effect could be exerted on the animal tissue.

These intentions the author considers as having been fulfilled by the employment of weak solutions of some of the vegetable supersalts of lead; such as the supermalate, saccharate, lactate, &c. The preparation, however, to which he gives the preference, is an acid saccharate, or, as he calls it, a *nitro-saccharate of lead.*

The salt, whichsoever it may be, must be moistened with a few drops of acetic, or of its own proper acid, previous to solution in water, whereby alone perfect transparency and activity are secured. He furthermore states, that the decomposing liquid should not exceed in strength one grain of the salt to each fluid-ounce of water, as the decomposing effect is in an inverse ratio to its strength.

Having by experiments which are fully detailed ascertained the chemical effects of the above class of decomponents on calculous concretions *out* of the body, the author briefly alludes to the case of three patients, in each of whom from four to eight ounces of these solutions had been repeatedly, for weeks together, introduced into the bladder, and retained in that organ without inconvenience for the space of from ten to fifty minutes.

It not being the intention of the author to enter into the medical history of these cases, he merely cites the above facts as sufficient to establish the principle originally laid down; namely, chemical decomposition of phosphatic calculi, by means of solutions so mild as to be capable of retention in the living human bladder without irritation or inconvenience.

2. "A Method of proving the three leading properties of the Ellipse and the Hyperbola from a well-known property of the Circle." By Sir Frederick Pollock, Knt., F.R.S., Her Majesty's Attorney General. Communicated in a letter to P. M. Roget, M.D., Secretary to the Royal Society.

In this communication, the author first demonstrates the well-known property of the circle, that if from a point in the diameter produced there be drawn a tangent to the circle, and from the point of contact there be drawn a line perpendicular to the diameter; and

if from any point in the circumference there be drawn two lines, one to the point without the circle, and another to the foot of this perpendicular, the former of these lines will be to the latter, as the distance of the point without the circle from the centre, is to the radius of the circle. By means of this property, and assuming that the ellipse is the curve whose ordinate, at right angles to its axis, is to the corresponding ordinate of the circle, described upon this axis as a diameter, in a constant ratio, the author proves the following propositions relating to this curve:—

1. The rectangle of the abscissæ is to the square of the ordinate, as the square of the semiaxis major to the difference of the squares of the semiaxis major and the excentricity.

2. The distance of any point in the curve from the focus, is to its distance from the directrix, as the excentricity is to the semiaxis major.

3. The sum of the distances of any point in the curve from the two foci is equal to the axis major.

By a method nearly similar to that employed for the ellipse, and assuming that the hyperbola is a curve in which the rectangle of the abscissæ is to the square of the ordinate, as the square of the ordinate in a circle, described upon the axis major as a diameter, is to the square of the subtangent, the author shows, first, that the distance of any point in the curve from the focus is to its distance from the directrix, as the distance between the foci is to the axis major; and secondly, that the difference of the distances of any point in the curve from the two foci is equal to the axis major.

3. "On the diurnal Temperature of the Earth's surface, with the discussion of a simple formula for ascertaining the same." By S. M. Drach, Esq., F.R.A.S. Communicated by John Lee, Esq., LL.D., F.R.S.*

The author investigates the several causes which influence the daily temperature of any point at the earth's surface. He employs the term *Thermal establishment* to denote the retardation of the effects of solar light caused by atmospherical conduction and by local circumstances, in the same manner that the term *Tidal establishment* has been used to express the local constant by which the astronomical effects on the waters of the ocean are delayed. After explaining the formation of the tables and diagrams given at the end of the paper, and detailing the conclusions derivable from them, the author enters into a review of the perturbing causes, investigates the analytical expression for the daily heat, and concludes with some observations on isothermal lines, on the influence of the friction resulting from the rotation of the earth about its axis, and on the agency of electricity.

March 2.—1. A paper was read, entitled, "On the laws of Individual Tides at Southampton and at Ipswich." By G. B. Airy, Esq., M.A., F.R.S., Astronomer Royal.

The author gives the results of his own personal observations of the tides at Southampton and at Ipswich, in both of which places

* See Phil. Mag., S. 3. vol. xx. p. 511.—Edit.

they present some remarkable peculiarities. In conducting these inquiries he obtained, through the favour of Colonel Colby, R.E., and Lieut. Yelland, R.E., the able assistance of non-commissioned officers and privates of the corps of Royal Sappers and Miners. He explains in detail the nature of his observations, and the method he pursued in constructing tables of mean results; and deduces from them the conclusion, that the peculiarities in the tides which are the object of his investigation are not dependent on any variations in the state of the atmosphere, but are probably connected with the laws which regulate the course of waves proceeding along canals.

2. A paper was in part read, entitled, "On the Special Function of the Skin." By Robert Willis, M.D. Communicated by John Bostock, M.D., F.R.S.

March 9.—1. The reading of a paper, entitled, "On the Special Function of the Skin." By Robert Willis, M.D. Communicated by John Bostock, M.D., F.R.S., was resumed and concluded.

The purpose which is answered in the animal economy by the cutaneous exhalation has not hitherto been correctly assigned by physiologists: the author believes it to be simply the elimination from the system of a certain quantity of pure water, and he considers that the saline and other ingredients which pass off at the same time by the skin are in too inconsiderable a quantity to deserve being taken into account. He combats by the following arguments the prevailing opinion, that this function is specially designed to reduce or to regulate the animal temperature. It has been clearly shown by the experiments of Delaroche and Berger, that the power which animals may possess of resisting the effects of a surrounding medium of high temperature is far inferior to that which has been commonly ascribed to them; for in chambers heated to 120° or 130° Fahr., the temperature of animals is soon raised to 11° or even 16° above what it had been previously, and death speedily ensues. The rapid diminution or even total suppression of the cutaneous exhalation, on the other hand, is by no means followed by a rise in the temperature of the body. In general dropsies, which are attended with a remarkable diminution of this secretion, an icy coldness usually pervades both the body and the limbs. A great fall in the animal temperature was found by Fourcauld, Becquerel and Breschet to be the effect of covering the body with a varnish impervious to perspiration; and so serious was the general disturbance of the functions in these circumstances, that death usually ensued in the course of three or four hours.

The question will next arise, how does it happen that health and even life can be so immediately dependent as we find them to be on the elimination of so small a quantity of water as thirty-three ounces from the general surface of the body in the course of twenty-four hours? To this the author answers, that such elimination is important as securing the conditions which are necessary for the endosmotic transference between arteries and veins of the fluids which minister to nutrition and vital endowment. It is admitted by physiologists that the blood, while still contained within its conducting channels, is

inert with reference to the body, no particle of which it can either nourish or vivify until that portion of it which has been denominated the *plasma* has transuded from the vessels and arrived in immediate contact with the particle that is to be nourished and vivified: but no physiologist has yet pointed out the efficient cause of these tendencies of the plasma, first, to transude through the wall of its efferent vessels, and secondly, to find its way back again into the afferent conduits. The explanation given by the author is that, in consequence of the out-going current of blood circulating over the entire superficies of the body perpetually losing a quantity of water by the action of the sudoriparous glands, the blood in the returning channels has thereby become more dense and inspissated, and is brought into the condition for absorbing, by endosmosis, the fluid perpetually exuding from the arteries, which are constantly kept on the stretch by the injecting force of the heart.

In an appendix to the paper, the author points out a few of the practical applications of which the above-mentioned theory is susceptible. Interference with the function of the skin, and principally through the agency of cold, he observes, is the admitted cause of the greater number of acute diseases to which mankind, in the temperate regions of the globe, are subject. He who is said to have suffered a chill, has, in fact, suffered a derangement or suppression of the secreting actiou of his skin, a process which is altogether indispensable to the continuance of life; and a disturbance of the general health follows as a necessary consequence. Animals exposed to the continued action of a hot dry atmosphere die from exhaustion; but when subjected to the effects of a moist atmosphere of a temperature not higher than their own, they perish much more speedily; being destroyed by the same cause as those which die from covering the body with an impervious glaze; for, in both cases, the conditions required for the access of oxidized, and the removal of deoxidized plasma, are wanting, and life necessarily ceases. The atmosphere of unhealthy tropical climates differs but little from a vapour-bath at a temperature of between 80° and 90° Fahr.; and the dew-point in those countries, as for example on the western coast of Africa, never ranges lower than three or four degrees, nay, is sometimes only a single degree, below the temperature of the air. Placed in an atmosphere so nearly saturated with water, and of such a temperature, man is on the verge of conditions that are incompatible with his existence: conditions which may easily be induced by exposure to fatigue in a humid atmosphere under a burning sun, or other causes which excite the skin while they prevent the exercise of its natural function. The terms *Miasma* and *Malaria* may, according to the author, be regarded as almost synonymous with air at the temperature of from 75° to 85° Fahr., and nearly saturated with moisture.

2. A paper was also read, entitled, "On the Cause of the reduction of Metals from solutions of their salts by the Voltaic circuit." By Alfred Smee, Esq., F.R.S., Surgeon to the Bank of England.

The reduction of a metal from its saline solution by the agency of voltaic electricity, has, the author states, been explained in three

different ways. By Hisinger, by Berzelius, and by Faraday it has been ascribed to the liberation of hydrogen in this process: Davy and others considered it as resulting directly from the attraction of the metal to the negative pole: and Daniell conceives that the metal [solution?] is directly electrolysed by the action of the voltaic circuit. The author found that the ends of copper wires, placed in a solution of sulphate of copper between two platina poles in the circuit, manifest electric polarity; so that while one end is dissolving, the other is receiving deposits of copper: he also found that platina was, in like manner, susceptible of polarity, although in a much less degree than copper, when placed in similar circumstances. With a view to determine the influence of nascent hydrogen in the voltaic reduction of metals, he impregnated pieces of coke and of porous charcoal with hydrogen, by placing them, while in contact with a metal, in an acid solution, when they thus constituted the negative pole of the circuit; and he found that the pieces thus charged readily reduced the metals of solutions into which they were immersed; and thence infers that the hydrogen is the agent in these reductions. From another set of experiments he concludes, that during these decompositions, water is really formed at the negative pole; a circumstance which he conceives is the chief source of the difficulties experienced in electro-metallurgic operations when they are conducted on a large scale, but which may be avoided by a particular mode of arranging the elements of the circuit so as to ensure the uniform diffusion of the salt.

The author obtained the immediate reduction of gold, platina, palladium, copper, silver and tin from their solutions by the agency of hydrogen contained in a tube, with a piece of platinized platina in contact with the metallic salt: nitric acid and persalts of iron, on the other hand, yielded their oxygen by the influence of the same agent.

The general conclusion which he deduces from his experiments is that, when a metallic solution is subjected to voltaic action, water is decomposed, its oxygen passing in one direction, and its hydrogen in the opposite direction; the latter element performing at the moment of its evolution at the negative pole the same part with respect to a solution of sulphate of copper, that a plate of iron or zinc would perform to the same solution.

March 16.—The following papers were read, viz.—

1. "On the import and office of the Lymphatic Vessels." By Robert Willis, M.D. Communicated by John Bostock, M.D., F.R.S.

That absorption is the special office of the lymphatic vessels was, until very lately, a universally received doctrine in physiology: but it is now admitted that if they exercise this faculty, it can be only to an inconsiderable extent; and physiologists of high authority have even denied that they possess any absorbing power at all. This last is the opinion of Magendie, in which the author concurs. So lately as 1841, Rudolph Wagner asserted that "neither anatomical nor physiological considerations render any satisfactory account of the import and office of the lymphatics," which thus, shorn of their ancient office, were repudiated as a superfluous apparatus in the

animal mechanism. The grand organs of absorption the author believes to be the veins; and a principal object of his paper is to point out the mode in which they acquire this remarkable faculty. The principal condition which this faculty of imbibition implies, is a difference in density between the contents of the vessels which are to absorb, and the contents of those which furnish the matter to be absorbed. If the several constituent materials of the body, both fluid and solid, were to remain in the same unaltered state, both chemically and physically, there could be no interchange among them: in order that mutual penetration may take place between two elements, the one must differ from the other: that which is designed to absorb must be, with relation to that which is to be absorbed, more dense; that is, must contain a smaller quantity of water in proportion to its solid ingredients. For the continuance of the delicate processes concerned in the access and removal of the nutrient fluids, it is necessary that a difference should be established between the arterial and the venous blood in respect of density. This purpose the author conceives is accomplished by the abstraction from the former of a portion of its water by the sudoriparous glands of the skin on the one hand, and by the lymphatic vessels on the other.

That the separation of the lymph from the blood is calculated to increase its density, is proved by its chemical analysis; lymph containing from 96 to 97 per cent. of water, and blood from 77 to 82 per cent. The author regards this separation of lymph from the blood as the result of a purely vital process of the same nature as that by which the saliva and the watery portion of the urine are secreted from the circulating mass. He considers that his views are supported by the anatomical distribution of the lymphatic system: for, on the principle that organs are found in the vicinity of the places where their office is wanted, the office of the lymphatics must be general, inasmuch as the system is general. These vessels may, in fact, be regarded as the essential element of an universally distributed gland. The mode in which the lymphatics are finally connected with the blood-vessels appears also to indicate that the object in view is to keep their watery fluid separate from the blood as long as possible; for, as is well known, they do not transfer their contents into the neighbouring veins, but pour their whole fluid into the superior vena cava at the moment it is about to enter into the heart.

The remarkable manner in which the lymphatic system is developed in some of the lower tribes of animals, whose bodies are encased in an impervious horny covering, such as turtles, lizards and serpents, is adduced in further corroboration of the author's views. He regards the serous membranes as contrivances for the accommodation of a great number of lymphatics; and the intimate connexion which the function of these vessels has with the life and nutrition of internal organs he thinks is shown by the remarkable amount of disturbance consequent on inflammation, or other morbid condition of serous membranes. Finally, the author adverts to the influence which the difference of endosmotic capability engendered

by the abstraction of a certain amount of water in the conrse of the circulation, (first between the blood corpuscles and the plasma in which they swim, and then between the liquor sanguinis and the containing channels,) must have on the capillary circulation, which he conceives it is calculated to facilitate.

2. "Further Observations on the descending fluids of Plants, and more especially the Cambium." By George Rainey, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author relates an experiment in proof of the sap descending from the upper to the lower part of an exogenous tree, through vessels which are continuous from the leaves to the roots; the course of these vessels being shown by the addition of a solution of iodide of potassium after they had taken up by absorption a quantity of a solution of acetate of lead. The fluids in these vessels are, he conceives, separated from the sap, which is ascending from the roots, only by the membrane of which they are composed. When the leaf-buds of a tree are vegetating, large separations are observable between the cells of the bark, and also between the bark and the wood; while no such separations are apparent when the leaf-buds are entirely inactive. These separations are various in size, and irregular in form; their parietes consist of rows of cells, piled up one above another, like the bricks of a wall: and their cavities all communicate with one another. From these and other anatomical facts, which are given in detail by the author, he concludes that the propulsion of the sap along the vessels, resulting from the operation of endosmose, will explain the descent of the cambium, which, being the nutritious portion of the vegetable fluids, corresponds in its nature to the chyle in animals.

March 23.—A paper was read, entitled, "Notice of an Extraordinary Luminous Appearance seen in the Heavens on the 17th of March, 1843," in a Letter to S. H. Christie, Esq., Sec. R.S., by Sir John F. W. Herschel, Bart., F.R.S.

Collingwood, March 17, 1843.

My Dear Sir,—This evening, at half-past seven o'clock, I received notice from one of my servants of a luminous appearance in the sky, visible towards the S.W., which I immediately ran out to observe, and which, as it differed in some remarkable particulars from any phænomenon of the kind I have ever before observed or seen described, I think it not unlikely to prove interesting to the Royal Society.

The evening was one of uncommon serenity and beauty: the moon, only thirty-eight hours after the full, having considerable south declination, was not yet risen. In consequence, the sun being already far enough below the horizon to leave only a faint glow of twilight in the west, the stars shone with unsubdued brilliancy, no cloud being visible in any quarter. Orion in particular was seen in all its splendour; and commencing below that constellation, and stretching obliquely westward and downwards, nearly, but not quite to the horizon, was seen the luminous appearance in question. Its general aspect was that of a perfectly straight, narrow band of con-

siderably bright white cloud, thirty degrees in length, and about a degree and a quarter, or a degree and a half in breadth in the middle of its length; its brightness nearly uniform, except towards the ends, where it faded gradually, so that to define its exact termination at either end was difficult. However, by the best judgement I could form, it might be considered as terminating, to the eastward or following side, at, or a very little beyond, the stars ι, κ, λ Leporis, which stars (being of the fifth, or at most 5·4 magnitude) were pretty conspicuously visible; from which circumstance the degree of brightness of the ground of the sky in that region may be well estimated. Between these stars and μ Leporis, the luminous band then commenced, involving neither of them, but more nearly contiguous to κ and λ than to μ. From thence its course was towards π Eridani, which star must have been covered by it, and was not seen; this judgement of its direction having been formed by noticing that it passed clearly above γ Eridani, and as clearly below and parallel to the direction of δ, ϵ Eridani, which two stars being dimmed by the vapours of the horizon and the twilight, were so little conspicuous as perfectly to account for π not having been noticed. At the point of its passage between γ and δ it was still considerably bright, and as it terminated with somewhat more abruptness at a point beyond ϵ (then about 12° high) than at its upper extremity, I am rather disposed to consider this end as somewhat curtailed by the vapours. Making no allowance, however, for this, and estimating its visible termination at a point on a celestial globe nearly opposite ζ Eridani (which star however was not noticed at the time), the length above assigned to the luminous band (30°) has been concluded by measurement on the globe.

I am thus particular in describing the course, situation and dimensions of the band, not only as terms of comparison with other observations of it, should any have been made, but for another reason, in which consists the peculiarity of the phænomenon, and which is my sole motive for making this communication. The above situation and course, relatively to those stars, *remained perfectly unaltered the whole time it remained visible at all*, which it did for upwards of an hour from the time I first saw it, *accompanying the stars in their diurnal motion*, until the preceding end at length was extinguished in the horizon vapours with the stars adjacent, and until the light of the rising moon dimmed and at length effaced the rest, though I apprehend its intrinsic lustre to have been in progress of diminution during the last quarter of an hour or twenty minutes.

I should not forget to mention, that neither in the north-west, nor elsewhere, were any streamers or other appearances of Aurora Borealis perceptible during any part of the evening. The only other luminous appearance, the milky way excepted, was that of the zodiacal light, which I have seldom seen to greater advantage in this climate, and which extended high enough to involve the Pleiades, then about 55° from the sun.

I have said that the general aspect of the phænomenon was that of a bright white cloud. In fact, my first impression was that such

was its nature; an impression immediately dissipated and ultimately converted into the contrary certainty by the following considerations and observed facts. For, in the first place, no ordinary cloud at such an angular elevation above the horizon could have received from the sun, even at the earliest hour when it was observed, anything like sufficient illumination to have presented so luminous an appearance; that luminary being then between 9° and 10° below the horizon, and the moon not yet being risen, even at eight o'clock, when I judged the light of the band by contrast with the increasing darkness of the ground of the sky to have attained its maximum, at which hour the depression of the sun was nearly 12°.

Moreover, 2ndly, about a quarter of an hour after the band was first observed, being then on the roof of my house and having a very uninterrupted view of the western horizon, I noticed the formation of a small streak of cloud about the same apparent altitude, somewhat to the north of the pyramid of the zodiacal light, and therefore nearer to the place of the sun below the horizon. The direction of this streak was horizontal, not oblique, and its hue black, not white. This cloud enlarged and became projected as a dark space within the zodiacal light, and soon after others of a less defined character formed elsewhere, all, however, without exception, dark instead of luminous.

3rdly. At the rising of the moon, about half-past eight, the light of our band, already probably on the decrease, was almost wholly effaced. On the other hand, by this time numerous lines and cirrous streaks of light cloud which had been for some time in progress of formation, and had been either wholly unseen before or only noticed by their effacing the stars behind them, became illuminated, and appeared as white streaks and patches, such as are usually observed in moonlight nights.

4thly, and lastly. Although the night was very calm, yet on watching narrowly the motions and changes of these real clouds with respect to the stars, they were perceived to *rise very slowly from the west, i. e.* in a direction nearly or quite contrary to that of the declining band.

From these united considerations, and from the extreme fixity of the band among the stars, I consider it impossible to regard it as a cloud illuminated by the sun through the medium of atmospheric refraction. The latter reason, too, is equally conclusive against its being classed with ordinary auroral bands and arcs, which, though they keep their position well enough to be regarded as at rest by a careless observer, yet, when compared with stars, are always perceived to be drifting, as it were, in some certain direction, or otherwise changing in figure and dimension.

If we look to an origin for this phænomenon beyond our atmosphere, we become involved in speculations, which, however interesting, it is not the object of this communication to enter into. On the other hand, its purpose will be answered if either it should be the occasion of eliciting corresponding observations of the same, or notices of similar phænomena already observed, or should lead

to increased watchfulness on the part of meteorologists to avail themselves of occasions (which perhaps occur oftener than we are aware) of noting anything analogous in future.

I have the honour to remain,
My dear Sir,
Your very faithful and obedient Servant,
J. F. W. Herschel.

Saturday, March 18, 1843.

P.S.—There having been no post today, and the above not having been finished in time for despatch last night, an opportunity is afforded me for stating that the phænomenon above described has again reappeared this evening, at the same hour and in the same situation, or rather a very little more to the north, so as to graze and partly to involve the stars κ, λ Leporis. It was also traceable in R. A. some little way beyond those stars on the following side. The horizon being more obscured by vapour tonight than last night, neither γ, δ, nor ϵ Eridani could be seen.

The fixity of this object among the stars on the 17th, induced me to express to a member of my family this morning an idea that it might possibly be seen again tonight, in which event its extra-atmospheric origin would become quite evident. If a thread be stretched on a celestial globe along the central line of the band as nearly as the above observations will enable us to fix it, and prolonged to meet the ecliptic, *it will strike on the actual place of the sun.* The inference seems almost unavoidable, that our band is no other than the tail of a magnificent comet, whose head at the times of both observations has been below the horizon *. I await, therefore, with extreme interest, the event of further observation, but although to afford others an opportunity of observing it, it will be necessary for me to make a more immediate and public announcement, I am still desirous to place on record my first impressions respecting so remarkable an appearance, in the mode originally intended, both as a mark of respect to the Royal Society, and as pointing inquiry to other luminous "streaks" and "columns" in the sky, which have been spoken of to me as having been seen during the last summer and autumn on more than one occasion, and which in point of fact caused me to desire every inmate of my family to give me immediate notice of the appearance of anything unusual in the heavens, and thus led directly to the observations above detailed.

A paper was also in part read, entitled, "Researches into the Structure and Developement of a newly discovered parasitic Animalcule of the Human Skin, the *Entozoon folliculorum*." By Erasmus Wilson, Esq. Communicated by R. B. Todd, M.D., F.R.S.

GEOLOGICAL SOCIETY.

[Continued from vol. xxii. p. 228.]

April 6, 1842.—A Memoir, entitled "A Second Geological Survey of Russia in Europe," by Roderick Impey Murchison, Esq.,

* See Phil. Mag., S. 3. vol. xxii. p. 323.—Edit.

Pres. G.S., F.R.S., M. E. de Verneuil, Member of the Geological Society of France, and Count Keyserling, was also commenced.

April 20.—The reading of the Memoir on Russia commenced on the 6th of April was resumed and concluded.

With the exception of a sketch of the Ural Mountains, to be given in a subsequent memoir, and of two short notices previously read, on the Freezing Cave of Illetzkaya Zatchita, and on the "Tchornoi Zem," or Black Earth*, the following abstract contains the chief results of a second examination of Russia in Europe. Following the same method as in the account of their first examination, the authors describe the depositary strata in ascending order, successively adding to or correcting their previous knowledge of each mass of deposits.

Silurian Rocks.—The boundaries of these the most ancient fossiliferous strata are more correctly defined than last year, and new localities are cited. The lowest subdivisions of blue shale and ungulite grit, which were previously spoken of in certain inland spots only, are now described in the sea-cliffs of the Baltic between Reval and Narva, as well as on the banks of the rivers Narva and Luga, in which situations, as in the tracts S. and S.E. of St. Petersburgh, they constitute the inferior masses or representatives of the Lower Silurian Rocks.

The Upper Silurian Rocks, chiefly composed of thin-bedded limestone, occupy the summits of the coast-cliffs in question, and the platform on which the river Narva flows from the lake Peicpus to a chasm worn by its own action, where it constitutes the picturesque falls above the Castle of Narva. It is believed by the authors that this water-fall has receded (like those of Niagara, in America, and other places,) in consequence of a solid tabular rock overlying less coherent strata, which have been undermined and have occasioned the subsidence of the superior layers. In addition, however, to these conditions, the wearing away of the vertical cliffs of the Baltic and the retrocession of the falls of the Narva, are supposed, by the authors, to have been accelerated by another cause, viz. the direction of the symmetrical joints in the overlying limestone. These joints present a number of salient and re-entering angles which are exposed on the surface of the impending cliffs, and when the softer supporting strata have been partially excavated, the dividing lines of these natural joints facilitate the fall of the calcareous beds into the abyss below.

Besides the chief masses of limestone which extend over a considerable tract in the province of Esthonia, (including the Isles of Oesel and Dago,) the authors advert to a separate tract near the small town of Schavli, in the government of Wilna, occupied by upper Silurian rocks, which they discovered in their journey to St. Petersburgh, and which they place as the highest member of the system, or above the principal masses of the Orthoceratite and Trilobite limestone and beneath the overlying old red or Devonian

* See Proceedings of Geol. Soc. of London, vol. iii. pp. 712–714; [or Phil. Mag. S. 3. vol. xxi. p. 357; and vol. xxii. p. 71.—Edit.]

formation. In this limestone fifteen species of fossils were observed, including *Pentameri*, *Terebratulæ*, and *Orthidæ*; and it is considered to be the representative of a calcareous rock which ranges to the north of Dörpat and Weissenstein, and is known at Oberpahlen, &c. Notwithstanding their almost perfectly horizontal position, the strata in the Baltic provinces of Russia indicate most clearly a passage from a lower horizon on the north to a higher on the south, where they are surmounted by the Devonian system.

In announcing a large accession of Silurian fossils to their former lists, the authors advert to the labours of Professor Eichwald, who after a personal examination of the coast-cliffs and of the Isle of Dago, has been sedulously occupied in describing many new species. They also dwell upon the important addition to their knowledge of new forms contributed by Dr. Wörth, the secretary of the Imperial Academy of Mineralogy,—forms which they purpose to figure and describe in the course of the ensuing winter; and they acknowledge their obligations to Colonel Helmersen and the officers of the School of Mines, for aiding them in their acquisition of fresh knowledge concerning the contents of these the most ancient deposits of the Russian empire. In their tabular list of fossils the authors give the following as characteristic of, and in part peculiar to, the Silurian rocks of Russia:—

Asaphus expansus (Dalm.*), *A. cornutus*, *Illænus crassicauda* (Dalm.*), *Amphyx nasutus* (Dalm.*), *Orthoceratites vaginatus* (Schloth.†), *Lituites convolvens* (Schloth.†), *Clymenia Odini* (Eichw.‡), *Terebratula Wilsoni*[1] (Sow. in Sil. Syst.), *T. sphæra* (Von Buch§), *T. camelina* (Von Buch§), *Orthis anomala* (Terebrat. Schloth.†), *O. Uralensis*[1], n.s., *O. Panderi*, n.s., *O. cincta* (Eichw.‡), *Leptæna imbrex* (Pand.¶), *Leptæna rugosa* (Dalm.‖), *Spirifer biforatus* (Terebrat. id. Schloth.), *S. lynx* (Eichw.‡), *S. æquirostris* (Terebr. Schloth.), *S. porambonites* (Von Buch§), *Pentamerus Vogulicus*[1], n.s., very near to *P. Knightii* (Sil. Syst.), *Crania antiquissima*, nob. (*Orbicula antiquissima*, Eichw.‡), *Lingula quadrata* (Eichw.‡), (*L. Lewisii*, Sil. Syst.), *Ungulites* (Pand.¶), *Obolus* (Eichw.), *Sphæronites aurantium* (*S. citrus*, His.**), *Hemicosmites pyriformis* (Von Buch), *Catenipora labyrinthica* (Gold.††), *Favosites Gothlandica*[1], ‡‡ *Favosites Petropolitana*, *Graptolites*, &c.

Devonian Rocks (Northern Zone).—By visiting Livonia and Courland some essential points of interest were added to the knowledge

* Om Palæaderna eller de sa Kallade Trilobiterna. Stockholm, 1828.

† Die Petrefactenkunde der Vorwelt. 8vo. Götha, 1820.

‡ Sur le Système Silurien de l'Esthonie. 8vo. St. Petersbourg, 1840.

§ Beiträge zur Bestimmung der Gebirgsformationen in Russland. 8vo. Berlin, 1840.

‖ K. Vet. Acad. Handl. 1827.

¶ Beiträge zur Geognosie des Russischen Reiches. 4to. St. Petersburg, 1830.

** Lethæa Suecica. 4to. Holmiæ, 1837.

†† Petrefacta Germaniæ. 1ter Thiel. fol. Dusseldorf, 1826—1833.

‡‡ Lamarck, Animaux sans Vertèbres, tome 2. 8vo. Paris.

[1] The fossils marked (1) occur in the Ural mountains only.

which the authors had previously obtained of the relation and contents of the old red or Devonian series. The central districts of Courland have been, for the first time, proved to contain rocks of this age charged with typical fossils, both fishes and shells. A section of the Düna river above Riga which exhibits some undulations of the strata, exposes siliceous limestones, subordinate to red and greenish shale; whilst the country between Riga and Dörpat is occupied by sands and marls. M. Pander, who now resides in this district, has collected a large and instructive series of its organic remains, chiefly from the banks of the river Aa; and among the Ichthyolites which they obtained from him, the authors recognised remains of *Coccosteus* and *Holoptychius* similar to those previously collected by them in the Waldaï Hills, and which Professor Agassiz has identified specifically with forms described by him from the old red sandstone of Scotland. Professor Owen has also identified among teeth from the collection of M. Pander, two or more varieties of the genus *Dendrodus* (Owen), equally characteristic of the old red sandstone of Scotland, one of them being indeed undistinguishable from the *Dendrodus* of that author, described from specimens found at Scat's Craig near Elgin.

In the marls and sands of Dörpat, Professor Asmus of the University at that place has collected and is describing certain gigantic bones, which were formerly supposed to belong to Saurians, but which, by their analogy to existing skeletons, he has shown to belong to fishes*. A single bone of one of these remains is nearly three feet long, and according to the estimate of Professor Asmus, the Ichthyolite of which it is a part must have had, when entire, a length of not less than thirty-six feet. The union of these fishes, some of the species of which, as above stated, are typical of the old red sandstone of the British Isles, with numerous fossil shells which have been found to characterize the beds of the Devonian age in England, Belgium, and the Boulonnais (an union was pointed out last year as resulting from an examination of the provinces of St. Petersburgh, Novogorod, Olonetz, &c.), is now more amply confirmed by reference to the structure of the north-western governments of Russia, through which the same system is spread.

Southern Zone of Devonian Rocks, or Geological Axis of Russia in Europe.—Previous to their visit to the central and southern regions of Russia, the authors believed, in common with their precursors, that the ascending order of the strata was continuous from the Baltic provinces on the north to the Black Sea and Sea of Azof on the south, with the exception only of the granitic rocks and carboniferous tracts of the southern steppes. They were undeceived, however, by discovering in the heart of Russia (Orel, Voroneje, &c.) a great domelike elevation, which is composed of rocks

* At the request of Mr. Murchison, Professor Asmus has prepared and sent to England duplicate casts of these the most remarkable and most gigantic fossil fishes ever yet discovered. One set of these has been given by Mr. Murchison to the British Museum, another to the Geological Society of London, and a third to Professor Agassiz.

loaded with Ichthyolites and Mollusks, all eminently characteristic of the Devonian system*. This mass sinks to the north under a great band of carboniferous rocks (Tula, Kaluga, &c.), the northern part of which was last year described as occupying the territory around Moscow and extending thence north-eastwards to the neighbourhood of Archangel: to the south it is lost under younger accumulations of secondary age. The dome of palæozoic rocks rising to an altitude of about 800 feet above the sea, was thus found to divide Russia into two distinct geological basins, viz. that of the carboniferous limestone of Moscow on the north, and that of the Jurassic, cretaceous and tertiary deposits on the south. One of the most remarkable features of this central mass consists in the lithological character of its rocks, as contrasted with that of formations of the same age, and containing the same fossils, in the northern governments; for whilst the latter in their range from the western borders of Lithuania to Olonetz and Archangel, including part of the Waldaï Hills (see last year's memoir, Phil. Mag. S.3. vol. xix. p. 492), are invariably made up of sands, sandstones, marls, and impure limestones, of prevailing *red* and *green* colours; their equivalents in Orel and Voroneje are *yellow* and *white* marlstones and limestones, the latter often in the state of magnesian limestone, and resembling in external aspect the Zechstein of Germany or the rocks of Sunderland in the British Isles. In addition to the characteristic fossils, enumerated last year from the great northern Devonian region, the central masses, particularly at Voroneje, have afforded many shells which have been published as typical of strata of the same age in Western Europe, such as *Spirifer Archiaci, S. Verneuillii, Leptæna Dutertrii, Productus productoides*, of the Boulonnais†, together with *Orthis crenistria, Productus spinulosus*, and *Aulopora, Favosites*, and other polypifers. It is indeed very remarkable, that in countries so distant from each other as the central region of Russia and the Boulonnais, twelve species at least of the fossils found at Voroneje should prove to be common to the rocks of the same age in both localities, and that in both instances the order of superposition should be so clear. The superior value, however, of the Russian sections of this division of the Palæozoic rocks over those in every other part of Europe, consists in the conjunction before adverted to and so generally observed in Russia, of Holoptychius and other fishes of the old red sandstone of Scotland and England, with the

* A part of the tract between Orel and Lichwin was examined by Colonel Helmersen during the same summer, and before the visit of the authors, and he also recognised the existence of Devonian rocks. The authors, however, were quite unaware of this circumstance when they first published their views on this point at the end of September 1841, in a letter addressed to Dr. Fischer de Waldheim, and it was on their arrival at St. Petersburgh only, that they found that Colonel Helmersen had come to the same conclusions as themselves, in respect to a portion of the country in question.—See *Bulletin de la Société Impériale des Naturalistes de Moscou, Oct.* 1841.

† See Mr. Murchison on the Boulonnais.—*Bulletin de la Société Géol. de France*, tome xi.

fossil shells characteristic of South Devon, the Boulonnais, and the Eifel*.

Carboniferous Limestone and Coal.—The lowest beds of the carboniferous system in Russia are, as stated in our first abstract (Phil. Mag. S. 3. vol. xix. p. 493), sands and shale with thin seams of coal, *Stigmaria ficoides*, &c. The authors examined a considerable tract occupied by these beds to the south of Tula and Kaluga, where many additional natural outcrops have been discovered by Colonel Olivieri, the mineral having the lignite or impure character of the beds of coal described last year in a similar position in the Waldaï Hills. These strata are, the authors conceive, of the same geological age as those of the great productive coal-field of Berwickshire, which equally underlies the mountain limestone.

By their recent labours the authors have divided the carboniferous limestone of Russia into three members. The lowest of these, generally a dark-coloured rock, is characterized by the presence of *Productus giganteus* and *P. Waldaicus* (near to *P. anomala*, Sowerby, &c.). The central mass is the well-known white limestone of Moscow, containing *Spirifer Mosquensis*, *S. resupinatus*, *S. glaber*, the *Chætetes radians*, *Euomphalus pentangulatus*, and many other fossils, some of which (such as *Productus antiquatus*, *P. comoides*) are found also in the lower division. Beds of compact, yellow, magnesian limestone occur in this central part of the carboniferous system, as well as bands of red and greenish shale or marl, and thin beds of pure siliceous flint graduating into ordinary limestone chert.

The third calcareous division is one which is not seen in the Waldaï or Moscow district, but which seems to surmount the before-mentioned divisions on their eastern flank at Velikovo and Kosrof, on the river Kliasma. Again, the lofty cliffs which occupy the banks of the Volga between Stavropol and Samara are almost exclusively composed of this member of the carboniferous limestone, which is there made up of myriads of *Fusulinæ* (the fossil bodies mentioned by Pallas as resembling grains of wheat), associated with *Euomphalus pentangulatus*, *Cyathophylli*, &c.

In a part of the coal region between the Dnieper and the Don, the authors detected a band of this *fusulina* limestone, in the same relative position which had been assigned to it in other parts of Russia, namely, in the upper part of the calcareous strata.

Carboniferous Region between the Dnieper and the Don, or Coal-field of the Donetz.—Whilst the central member of the carboniferous limestone of the northern parts of Russia (Moscow basin) contains no coal, and the upper beds on the Volga are equally void of it, rocks of the same age in the South of Russia, or on the banks and in the neighbourhood of the river Donetz, are in parts eminently productive of good bituminous as well as anthracitic coal. Among the sections described, one from Karakuba, on the river Kalmiuss, to the neighbourhood of Bachmuth, shows a regular succession, in ascending order, from beds of conglomerate and red

* The large scales of *Holoptychius Nobilissimus* were found by the authors at a locality called Kipet between Lichwia and Bielef.

sandstone, forming the base of the carboniferous system, through various bands of limestone, alternating with many courses of sandstone and shale with numerous seams of coal.

In this wide carbonaceous tract, coal is extracted by the imperial government at two spots only. These pits were first opened in the last century, by Mr. Gascoigne and a small company of English miners, formerly employed by the Russian government. The shaft section at Lissitchi Balka, the chief of these places, and situated to the north of the iron foundries of Lugan and to the east-north-east of Bachmuth, clearly shows that all the best seams of coal of this tract are subordinate to the central part of what English geologists call the mountain limestone. Including small and profitless seams, twelve beds of coal occur at this locality, seven of which are extracted for use. The greater part of the coal is of fair quality, and some is exceedingly good and chiefly bituminous; and all these beds, with a great amount of shale and sandstone occupying a thickness of 800 English feet, are interlaced with thin courses of limestone, which are charged with *Spirifer Mosquensis, Productus antiquatus, Orthis lata, O. planissima, Bellerophon, Turritella, Pecten, Nautilus*, and a small Trilobite, thus leaving no doubt that the coal is subordinate to the same series of beds which in the North of Russia, beyond the great Devonian axis before described, is void of the mineral, and yet contains the same fossils. In examining these tracts of coal, the authors perceived a close analogy between them and those of the North of England. In the South of England, as in the North of Russia, no coal occurs in the lower or calcareous division of the system; but in Yorkshire, Durham and Northumberland, sandstone and shales are interpolated and the mountain limestone is expanded, as on the Donetz, into a great complex series (Yoredale Rocks of Phillips), including seams of coal.

In the mineral composition of this carboniferous tract there is a striking analogy to the condition of the great British coal-field of South Wales; for one end of the tract contains anthracitic, and the other bituminous coal, though the strata are, it is believed, of the same age. In the Russian case, the anthracitic masses occupy the eastern end of a tract, the major axis of which trends from west-north-west to east-south-east, and the bituminous coal is on the west. In the tract where the anthracite prevails, the limestone seems to thin out, and there are consequently fewer fossils.

Unlike the flat and untroubled regions of northern and central Russia, this carboniferous tract is often highly dislocated, and is everywhere thrown into broad and rapid undulations. In the chief mines at Lissitchi Balka the strata dip about 20°, and are therefore easily worked and drained; but at Uspenskoi, near Lugan, the beds, which are neither so numerous nor so good as at the former place, are inclined at 50°, and even at 70°, and are full of extensive faults.

The carbonaceous strata (often worked by the small proprietors and Cossack and Russian peasants) are described in several places, and the same geological relations are shown to prevail, the coal

beds being stated in all cases to be subordinate to the mountain limestone series, whilst certain overlying shales, sandstones, &c., which were observed in one corner of the district, contain few or no traces of coal.

At the western extremity of this region, the coal-bearing strata thin out into sandy masses, which repose unconformably on certain highly inclined quartzose, gneiss and granitic rocks, that appear on the banks of the river Voltchia, and extend to the Dnieper and the cataracts of that river near Ekaterinoslaf. To the south-west, near Karakuba and towards Mariopol, in a tract occupied by Greek colonies, similar primary rocks appear, penetrated both by granite and porphyry, whilst to the south-east and north the whole carbonaceous region is overlapped partially by red sandstone with gypsum, as near Bachmuth, but more generally by cretaceous and tertiary rocks. The former, in the state of white chalk, occurs in a large zone in the north, and in a smaller band at the southern limits of the coal tract.

The dislocations and upheaval of the subjacent rocks extend to some distance to the north of the chief carbonaceous masses; for at Petrofskaya, considerably to the north of the nearest outcrop of the chief coal-field, coal with carboniferous limestone is upcast to the surface in highly inclined positions, surrounded by nearly horizontal strata of the Jurassic and cretaceous epochs, and generally so obscured by drift and clay, that it is well seen in one ravine only. Coal, however, has been detected at adjacent places in sinking for water.

The uppermost members of the carboniferous system are not observable in the North of Russia, or in the Moscow basin, where Jurassic strata repose at once upon true carboniferous limestone; but in the southern coal-tract, just alluded to, there are, as before said, beds of shale and sand which overlie this limestone series, and yet are unproductive of coal (north of Gorodofka). On the western flanks of the Ural mountains, however, as will be shown in the next memoir, to the east of Perm, and at Artinsk, are sandstones and conglomerates with plants passing occasionally into calcareous grits with Goniatites, which, as seen on the banks of the Tchussovaya and near Artinsk, are superior to the great carboniferous limestone. Very thin courses of coal only are observed at intervals in this upper member of the system, and the Goniatites which it contains belong, as a whole, to that division of the family which characterizes the uppermost member of the carboniferous limestone and certain coal-fields (Coalbrook Dale) of Western Europe. There is a considerable development of this subdivision on the flanks of the Guberlinski hills, and partially on the south-western edges of the Ural east of Orenburg.

Permian System. (*Zechstein* of Germany—*Magnesian limestone* of England.)—Some introductory remarks explain why the authors have ventured to use a new name in reference to a group of rocks which, as a whole, they consider to be on the parallel of the Zechstein of Germany and magnesian limestone of England*. They

* "I have recently been informed by M. A. Erman, that an erroneous

do so, not merely because a portion of the deposits in question has long been known by the name "grits of Perm," but because, being enormously developed in the governments of Perm and Orenburg, they there assume a great variety of lithological features, and contain the bones of thecodont Saurians and certain fishes, also a more copious fauna and flora than have ever been observed in their equivalents in Western Europe.

The Permian rocks of Russia which occupy so vast a region to the east of the river Volga, *i.e.* in the governments of Kasan, Viatka, Perm and Orenburg, are composed of white limestone with gypsum, red and green grits with shales and copper ores, magnesian limestones, marl-stones, small conglomerates, red and green sandstones, &c. By examining numerous natural sections between the neighbourhood of Sviask, Kasan, and Samara, upon the west, and the carboniferous limestone on the edge of the Ural mountains on the east, the authors have come to the conclusion, that however the lithological sequence may vary in different tracts, the whole of the vast region alluded to is occupied by deposits which belong to one class or zoological system of deposits. Thus, though the limestones are sometimes white, sometimes yellow and pure magnesian, and oftentimes pass into marl and marlstone, all of which can be observed to inosculate with strata of red sandstone, conglomerate, &c., the same fauna pervades the whole group. The Mollusca and Polypifers are clearly of a type intermediate between those of the carboniferous limestone and those of the Trias or new red sandstone group of Continental geologists. Among the most characteristic of these fossils may be enumerated *Productus horrescens*, n.s., *P. Cancrini*, n.s., *Spirifer lamellosus* (L'Ev.), *Terebratula elongata* (Schloth.), *T. Roysii* (L'Ev.) (*T. Roysii*, L'Ev. = *Atrypa pectinifera*, Sow. Min. Conch. No. 107), *Natica variata* (Phil.), *Modiola Pallasii*, n.s., *Gervillia lunulata* (Phil.), *Ostræa matercula*, n.s., *Corbula Rossica*, n.s., *Avicula Kasaniensis*, n.s., *A. antiqua* (Schloth.), *A. cheratophaga* (Schloth.), *Lingula parallela* (Phil.), *Limulus oculatus* (Kutorga), *Cytherina*; with *Retepora flustracea*, *Gorgonia*, *Millepora*, &c. &c.

In the conglomerates and sandstones, fishes have been found, some of which belong to the genus Palæoniscus, so characteristic of the Zechstein and magnesian limestones; and the Saurian bones, portions of which have been figured by M. Kutorga, and more perfect remains of which have been described by Professor Fischer von

view has been communicated in my anniversary discourse, respecting the first use of the word 'Zechstein' in reference to the deposits of Perm, that term having been used, as he assures me, *by German miners*, who visited Russia long ago, though no proofs have been since offered to sustain its application in *a geological sense*. I also take this opportunity to state, that through a misapprehension of his views, derived from a perusal of the Bulletin de la Société Géologique de France, I have been led into a mistake in supposing that M. Erman believed a large portion of the Russian rocks, now shown to be carboniferous, to belong to the Jurassic epoch. I willingly adopt this correction of my views in reference to the distinguished modern explorer of Siberia and Kamschatka."—R. I. M., Sept. 1842.

Waldheim (*Rhopalodon Mantellii*, Fisch.), have been pronounced by Professor Owen to belong to the class of thecodont Saurians of that author (See Report on Saurians to the British Association, 1841, p. 153).

Certain plants of this great deposit have been figured by M. Kutorga, and referred by him to the carboniferous epoch; others collected by Major Wangenheim Von Qualen have been named by M. Fischer de Waldheim, who, as well as their discoverer, felt great difficulty in forming any decisive opinion respecting the age of the strata in which these fossils occur. Having examined the localities and sections, the authors convinced themselves on the spot, that all these plants are of intermediate character between those of the carboniferous and triassic æras*. These vegetables of the Permian system, and many undescribed species of shells with which they are associated, will be figured in a forthcoming work on the geology of Russia, and for this purpose M. Fischer has kindly contributed some beautiful drawings of new genera and species which he had prepared at Moscow.

The publication of these new species will show that the epoch of the Zechstein was characterized by a flora peculiar to it. These fossil plants, although generally appearing to constitute an independent flora, offer some analogies in form to a few species belonging to the carboniferous series: one species cannot easily be distinguished from the coal-measure plant, *Cal. Suckowii*, which Brongniart considers to be very variable in form and to have a great geographical range. Among the characteristic forms may be mentioned the *Calamites gigas*, *Neuropteris Wangenheimii*, *N. salicifolia*, *Odontopteris Strogonovii*, *Sphenopteris erosa*, *Noeggerathia undulata*, and some other species to be described.

These plants are sometimes accompanied by thin courses of coal and lignite, which near Perm have some of the external characters of poor coal-fields. But while the carbonaceous appearances are evanescent and local, the fossil stems and leaves are very general indicators of the presence of copper ore, which, in the form of gray oxide and green carbonate, is often copiously disseminated through the vegetable matter, or arranged around the thicker branches in masses, from which it extends in fine filaments into the adjacent sands or marls. In all cases, the copper ores of this region occur in laminæ, inosculating with the other regular strata, in which respect they differ essentially from the chief copper ores of other countries. They are, in fact, regenerated ores, formed, it is conceived, by cupriferous streams and currents that flowed from the adjacent Ural mountains, which, it will be shown, were, during very early periods, the site of great copper veins†.

As a solution of copper which was let loose by accident in modern

* Mr. Morris, who has undertaken the description of the new species of these plants, completely confirms the views of the authors. (See letter of Mr. Murchison, dated Moscow, October, 1841. Phil. Mag. S. 3. vol. xix. p. 418.)

† Among the mineral analogies between the Permian rocks and those of

times upon an adjacent peat bog in North Wales specially affected and impregnated the vegetable fibre in preference to the accompanying soil, so is it conceived that the forests washed into the sea in which the Permian deposits were accumulated, attracted around them the cupriferous matter contained in the transporting currents. This point will be reverted to in the subsequent sketch of the Ural mountains.

The general succession of these Permian deposits is then described on several parallels of latitude between the Ural and the Volga, and also their outliers in the steppe between Orenburg and Sarepta; and it is shown, that this vastly extended and diversified system, containing not only copper deposits but also large masses of gypsum, rock-salt and copious salt-springs, lies in an enormous trough bounded on the north and east, and south-west, by the carboniferous limestone on which it reposes.

By their examination during the past year, the authors have cleared away some difficulties which obscured their former views. By reference to the abstract of their first memoir (vol. xix. p. 492), it will be seen, that they considered (though with much hesitation) certain limestones and beds of gypsum which occupy cliffs upon the Dwina to the south of Archangel, and extend to Pinega and towards Ust Vaga, to be upper members of the carboniferous limestone. By a comparison of the *Producti* and other fossils, and the great masses of gypsum which they contain, these northern beds are now brought into direct identification with the true Permian or Zechstein deposits. In the south-western termination of this vast basin near Samara, the Permian rocks, particularly at Usolie, rest in patches of a dolomitic conglomerate upon the steep escarpments of the carboniferous limestone, out of the materials of which they have been formed, and do not present that regular succession which they exhibit when followed westwards from the slopes of the Ural chain. It is also observed, that though gently undulating or horizontal over all the lower regions, these rocks, on approaching the Ural mountains, are occasionally thrown into anticlinal axes of some length, parallel to the direction of the palæozoic rocks of the adjacent chain.

In a sketch of the outliers in the Steppe of the Kirghiss, the base of the insulated hill of Monte Bogdo is shown to consist of a member of the Permian group, surmounted by fossiliferous limestone which probably belongs to the Jurassic system; and it has before been shown that the rock-salt of Iltetzkaya Zatchita *, south of

the magnesian limestone it appears, from Professor Sedgwick's description of the latter, that traces of lead ore and also of copper, are found in it in small quantities, which that author considers to have been derived from the large mineral masses of the same in the surrounding and more ancient carboniferous limestones. Lead is also worked in the dolomitic conglomerate of the Mendip Hills, where it is associated with calamine. See memoir of Mr. Conybeare and Dr. Buckland, Geol. Trans., 2nd Series, vol. i. part 2. p. 293; also Mr. Weaver's memoir, *ibid.* p. 367.

* Proceedings, vol. iii. p. 695, [or Phil. Mag. S. 3. vol. xxi. p. 357.]

Orenburg, is subordinate to this system, in which indeed the greatest saline springs of Russia occur.

Red Sandstone, Marl, &c.—It is with hesitation that the authors make any separation between the Permian deposits and certain red and green sandstones, marls, marlstones and tufaceous limestones, which occupy the central parts of the great trough above described; still less can they strictly identify them with the bunter sandstein, new red or trias of West Europe.

It is however a fact, that the Permian rocks with their peculiar fossils are seen near Sviask, on the west of Cazan, to pass under red and green marls and impure limestones, which extend over a wide region by Nijny Novogorod, Juriavetz and Viasniki on the west, and to Totma and Ustiug on the north. In no part of the region so defined (and most of which the authors examined on a previous occasion), have any fossils typical of the Permian age been discovered, though the deposits in question abound in limestones generally of a tufaceous character. The gypsum which occurs in this member, differs from the massive white alabaster of the inferior rocks, and is usually in the form of small concretions of fibrous structure, often of brownish and pinkish colours. At only Viasniki on the Kliasma could the authors detect any traces of fossils, and these are minute *Cypridæ*, associated with apparently flattened *Cyclades?* which are imbedded in blood-red marl. The thick cover of detritus which is spread over a very large area, obscures the junction of these red deposits with the eastern edges of the carboniferous limestone of the Moscow and northern regions. Whatever may be the precise age of the uppermost beds of these red deposits in reference to other strata in Europe, it is clear that a considerable portion of the full geological succession is wanting in Russia, for in various points upon the Volga, Jurassic shales are seen to repose on the denuded surface of these red deposits.

Jurassic System.—In the sketch resulting from their survey in 1840 (vol. xix. p. 495), Mr. Murchison and M. de Verneuil were disposed to view certain deposits of shale and sand with concretions, which in some places overlie the last-mentioned red deposits, and in others rest at once on the carboniferous limestone, as the equivalents of the lias and lower oolites. This opinion is now modified, a more extensive survey having led to the belief that true lias does not exist in Russia; but that the shale beds in question, whether studied in sections on the Moskwa near Moscow, on the Volga between Kostroma and Jurievetz, or at numerous localities in the governments of Simbirsk, Saratof and Tambof, are truly the equivalents of the strata from the inferior oolite to the Kimmeridge clay, inclusive, of English geologists.

It is this Jurassic group which is traceable at intervals so far to the north-east, and which has been found by Capt. Strajesski as far as even 65° N. lat. on the eastern flanks of the Ural chain.

The upper members of the Jurassic system, as exhibited in the South of Russia, near Izium, where they were first recognised by Major Blöde, differ both lithologically and zoologically from the

dark shales and sands of the northern and central regions. They are chiefly light-coloured limestones and marls, and are charged with large Ammonites resembling those of the Portland rock with *Trigonia clavellata*, *Nerineæ*, and other types closely allied to those which occur in the upper oolites of Great Britain and the Continent.

Cretaceous System.—This system is very considerably developed in the central and southern tracts of Russia. In the government of Simbirsk, where it has been closely studied and its fossils carefully collected by M. Jasikof, it surmounts the Jurassic series, and the same order may be seen in the governments of Sarátof and on the banks of the Donetz near Izium.

Though the lithological sequence of the strata differs from that of the British Isles, the system, as a whole, bears striking analogies to that of the same age in Western Europe. The white chalk, for example, and many of the fossils which it contains, including *Inoceramus Cuvieri*, *Belemnites mucronatus*, and *Gryphæa vesiculosa*, is absolutely undistinguishable from that of France and England; but in the localities seen by the authors, it did not offer the same subjacent succession of gault and lower greensand as in Western Europe, though at Kursk the white chalk reposes on hard concretionary sandy ironstone, somewhat resembling the clinkers of the lower greensand of England. Nor are there any evidences of the existence beneath the cretaceous rocks of the "Système Néocomien" of the French geologists. Associated however with the white chalk, the authors observed, particularly between Sarátof and Tzaritzin, many beds of marl and siliceous clay-stone, in which bodies like *Alcyoniæ* were prevalent, and at Kursk they found that the white and yellowish subcalcareous marls which closely overlaid the white chalk contained a Belemnite, as well as certain polypifers common to the true white chalk of other parts of Russia (Volsk), and hence they concluded, that some of these overlying marls are possibly the representatives of the Maestricht beds of Europe.

The white chalk alone has been pierced to a depth of upwards of 600 feet by an artesian shaft at the iron forges of Lugan, in Southern Russia, in which tract the deposit lies unconformably on the uplifted edges of the carboniferous rocks.

Tertiary Deposits.—The tertiary strata, as separated from diluvial and alluvial accumulations, are little known in the North of Russia, with the exception of the shelly strata of post-pliocene age which have been described in the government of Archangel, vol. xix. p. 495.

The lowest tertiary beds which the authors personally examined, are the marls with concretions forming clifts at Antipofka, on the right bank of the Volga below Sarátof, where they were first noticed by Pallas. Among these shells are several species undistinguishable from those published by Sowerby from the London clay of Bognor and Hants, such as *Cucullæa decussata*, *Venericardia planicosta*, *Calyptræa trochiformis*, *Crassatella sulcata*, *Turritella edita*, &c.

The middle tertiary or miocene strata are spread, it is well known, over large tracts in Volhynia and Podolia, in which countries they have been described or alluded to by Prof. Eichwald, M. Dubois de

Montperoux, Major Blöde, and others. Distinctions are, however, drawn between the more ancient tertiary strata, such as those of Antipofka and other places, and the recent Caspian shelly sands which cover the Steppes, the former having constituted a portion of the ancient shores of a more widely spread Caspian sea. The authors also entirely discard from residuary phænomena due to the presence and retirement of these Caspian waters, the existence of certain great subterranean masses of rock-salt and salt-springs which issue from the bowels of the earth, both of which have their seat in purely marine deposits of much higher antiquity, chiefly Permian, and which can never be referred to the desiccation of comparatively modern, brackish, inland seas.

The pliocene and post-pliocene strata, occupy a very large region in Southern Russia. The inferior division of this group is well exposed in the lowest part of the cliffs at Taganrog, on the sea of Azof, where beds of white and yellow limestone contain several species of Cardium, a Buccinum and large Mactræ, all of marine origin. The superior members, often reposing on sands and siliceous grits, constitute the widely spread "Steppe limestone," in which are many remains of Mollusca that must have lived in brackish seas.

These beds, as seen at Novo Tcherkask, the capital of the Don Cossacks, and adjacent places, are considered to be the extension of similar shelly deposits in the Crimæa and the neighbourhood of Odessa, described by M. de Verneuil (See Trans. Geol. Soc. of France, vol. iii. p. 1.).

The vast flat steppes of Astrachan traversed by Count Keyserling, who rejoined his companions at Sarepta, are proved, as suggested by Pallas, to have been the abode of the adjacent Caspian Sea at a comparatively modern period; and in confirmation of this view, it is stated, that not only the low country is covered with shells, but that the cliffs at Monte Bogdo, which rise out above this steppe, are also corroded to a certain height in the same way as sandstones of similar nature are affected by the surge of the present seas.

Superficial Detritus, Bones of Extinct Mammalia, Northern Boulders, &c.—It is shown that the mammoth alluvia are analogous to those of other countries in indicating, over large areas, a period when elephants, rhinoceroses and other gigantic animals of species now extinct, inhabited the surface of the earth not far from the spots where they are now interred, their bones, as demonstrated by their condition as well as by the matrix in which they lie, not having undergone distant transport. This subject will be again considered in a sketch of the Ural mountains, but in the mean time, lists of the animals, some of them peculiar to Russia, which are preserved in the museums of Moscow and St. Petersburgh, were given.

Lastly, new data are offered in respect to the southernmost limit of the northern blocks described on a previous occasion (vol. xix. p. 496), and their further advance to the south in some situations than in others, is attributed to the form of the present continent of Russia in Europe, nearly all of which, it is presumed, was under the sea during the distribution of these boulders.

The authors adhere to the opinion previously expressed by them, that such blocks were transported to their present positions by huge floating icebergs, arrested, in some instances, by rising grounds and hills at the bottom of the then sea, and in others permitted to advance further south by longitudinal depressions, which are traceable in the present configuration of the land. Proofs are given that in many instances blocks of trap and quartz rock advance to quite as southerly latitudes as those of granite, and that all these blocks can be traced back to their parent rocks in Russian Lapland and the northern parts of Russia in a north-north-westerly direction, the currents by which they were transported having therefore been directed to the south-south-east. The black earth or Tchornoi Zem, which forms the highest deposit of the central and southern regions of the empire, has been described in a previous memoir (See *antè*, vol. xxii. p. 71).

A large geological map of Russia in Europe, coloured by the authors, and numerous sections and collections of fossils, illustrated this communication, and it was announced that other conclusions respecting the structure of Russia would follow the description of the Ural Mountains*.

CHEMICAL SOCIETY.

(Continued from vol. xxii. p. 323.)

March 7, 1843.—The following communications were read:—

71. "On the Astringent Substances" (continued), by John Stenhouse, Ph. D.†

72. "On Æthogen and the Æthonides," by William H. Balmain, Esq.

On the 6th of December, 1842, I communicated to the Society the discovery of a new compound of nitrogen and boron which was named "Æthogen," and which, like cyanogen, combined with the metals‡. At that time hopes were held out that I should be able to furnish the Society with an analysis of æthogen and the results of further experiments, but I am still without the means of doing the former, and have been prevented by illness from working much at the latter. However, some experiments which I have been able to make have brought out very easy processes for preparing æthogen and the æthonides, which may be interesting to chemists, and will place at their disposal a ready means of obtaining these very stable compounds which may prove powerful agents.

Æthogen was originally obtained by heating together a mixture of boracic acid and melon, and the principal difficulty attendant upon the process was the previous preparation of the melon. An attempt

* On the Geology of Russia, see also Mr. Murchison's recent Address to the Geological Society, inserted in our Supplement to vol. xxii. published with the present Number.—EDIT.

† This paper, together with all those read before the Chemical Society, of which abstracts do not appear in these Proceedings, or which are not otherwise referred to, will be given entire in future Numbers of the Philosophical Magazine.—EDIT.

‡ Mr. Balmain's former communication was given in our last volume, p. 467.—EDIT.

having been made to form melon by heating together bicyanide of mercury and sulphur, it appeared that melon was formed, but was with difficulty separated from the sulphuret of mercury which accompanied it; but as the presence of the sulphuret of mercury does not interfere with the formation of æthogen from a mixture of melon and boracic acid, that substance may be obtained by simply heating together 5 parts of sulphur, 58 of bicyanide of mercury and 7 of anhydrous boracic acid, or by heating together sulphocyanogen and boracic acid. Having an easy process for preparing æthogen, it was advisable in the next place to have a more ready method of forming the æthonides than that of heating together æthogen and the metals, which is a long and uncertain process, and an attempt was made to form æthonides by heating æthogen with the sulphurets of the metals. As might be expected from the stability of æthogen and its strong affinity for the metals, the æthogen directly displaced the sulphur and formed the æthonide. Upon further experiment it was proved that the æthonides might be made by heating sulphur, bicyanide of mercury and boracic acid with the metallic sulphurets. The proportions should be such as would give rise to the presence of 2 atoms of the metallic sulphuret, 2 atoms of boracic acid (supposing its composition to be BO_3), 3 atoms of cyanogen, and 3 atoms of free sulphur.

The æthonides when thus formed are not quite pure, but may be readily purified by boiling with a mixture of nitric and muriatic acids and afterwards washing carefully. In this way æthonides of sodium, iron, copper and lead have been formed. Common galena was used for the æthonide of lead; and for that of iron, iron filings and an additional quantity of sulphur. These four æthonides are *all* perfectly white and infusible; before the blowpipe they yield the very beautiful phosphorescent light alluded to in a previous communication, and in all respects resemble the æthonides of potassium, zinc, lead and silver which were described as being made by the other processes.

In conclusion, I beg to draw the attention of the Society to the remarkable stability of these compounds and the very strong affinities of æthogen.

Æthogen attracts moisture from the air with great avidity, and decomposes it so rapidly, that a portion of æthogen which I have kept in a moderately well-stoppered bottle smells strongly of ammonia.

The want of means must still be my apology for not furnishing the Society with a quantitative analysis, but if any member of the Society will undertake one, I shall be most happy to supply him with a fair specimen of æthogen.

73. "On the Exhalation of Carbonic Acid from the Human Body," by E. A. Scharling, Professor of Chemistry in the University and Polytechnic School of Copenhagen. Communicated by S. Elliott Hoskins, M.D.

With the view of ascertaining the quantity of carbonic acid exhaled during the twenty-four hours, as well from the lungs as from the general surface of the body, Professor Scharling undertook the following experiments on six individuals, viz. four males and two females.

The subjects of experiment were confined in an air-tight box, wherein they were perfectly at their ease, being enabled to speak, eat, sleep, or read without inconvenience; a constant current of atmospheric air was admitted into the box, and the deteriorated gases abstracted by means of an air-pump. The air withdrawn was conducted into a proper arrangement of bottles, some containing sulphuric acid, others a solution of caustic potash. The quantity of carbonic acid, both previous to and subsequent to each operation, was carefully ascertained, by being received into three graduated tubes.

The results were as follows:—

1st. The Professor himself, aged thirty-five years, exhaled 219 grammes* during twenty-four hours, seven of which were spent in sleep.

2nd. A soldier, twenty-eight years of age, exhaled 239·728 grammes = 8·45 oz.

3rd. A lad of sixteen, 224·379 grammes = 7·9 oz.

4th. A young woman, aged nineteen, 165·347 grammes=5·83 oz.

5th. A boy nine years and a half old, 133·126 grammes = 4·69 oz.

6th. A girl of ten, 125·42 grammes = 4·42 oz.

In the two last cases the period allotted to sleep was nine hours.

From these experiments the Professor deduces that males exhale more carbonic acid than females, and children comparatively more than adults. He also finds that less of this gas is given off during the night than during the day; and that in certain cases of disease, which he does not specify, less carbonic acid is formed than during the healthy state. He is thence induced to hope that attention to this point may ultimately throw some light on certain forms of disease.

It will be interesting to compare these results with Liebig's views, as well as with the experiments which have recently emanated from the French Académie des Sciences.

March 21.—Col. Yorke exhibited a specimen of magnesium obtained by voltaic action on the chloride of magnesium.

The following papers were read:—

74. "On Theine," by John Stenhouse, Ph. D.

75. "Observations on M. Reiset's Remarks on the New Method for the Estimation of Nitrogen in Organic Compounds, and also on the supposed part which the Nitrogen of the Atmosphere plays in the Formation of Ammonia," by Heinrich Will, Ph.D. (See Phil. Mag., S. 3. vol. xxii. p. 286.

March 30.—Anniversary Meeting, the President, Thomas Graham, Esq., F.R.S., in the Chair.

The following Report of the Council was read by the President, and subsequently ordered for publication:—

Report of the Council made to the Chemical Society of London, March 30, 1843.

The completion of a second year of the Society's existence in circumstances of increasing prosperity enables the Council to congra-

* = 7·72 oz. avoirdupois.

tulate their fellow-members on the positive attainment of the principal objects for which they are associated. The Society continues to be augmented in numbers and influence by the election of new Members, and has been well supported by contributions of original papers read at its meetings. The papers presented appear to increase both in number and value; and any apprehension of a want of papers, which formerly existed, has been in a great measure dispelled by the experience of the last Session. It is now sufficiently evident that ample materials exist in England for a Chemical Society, and you have furnished unquestionable proofs of the utility of such a Society in its power to advance the cultivation of chemical research in the country.

Thirty-one Members have been elected into the Society since the last Anniversary. Our present numbers are—77 Members resident in London, 57 Members resident in the country, or "non-resident" Members, 10 Associates, and 3 Foreign Members, making a total of 147 Members, with an annual income of £211.

The Society has thus early in its career to deplore the loss by death of two Members.

Henry Hennell, Esq., F.R.S., who took an active part in the establishment of the Society, and was a member of the Council first elected. Mr. Hennell will ever hold an honourable place in the history of chemistry, as the discoverer of sulphovinic acid*, one of the earliest achievements in organic chemistry, and which has since formed the starting-point for numerous important inquiries. Mr. Hennell was destroyed by a lamentable accident, which no intelligence could have foreseen, in the discharge of his professional duties as Chemical Operator to the Apothecaries' Hall, in the 45th year of his age. The shock of this deplorable event still unfits us from calmly estimating the scientific merits and highly amiable character of our lost friend. And

Mr. Henry Inglis, of Kincaid Print Works, near Glasgow, who, besides cultivating successfully the chemistry of calico-printing, was distinguished for his accurate knowledge of the general science, in the progress of which he took much interest. Mr. Inglis, whose constitution was always delicate, did not outlive his 43rd year.

At the conclusion of last Session the Council made a new arrangement with the Society of Arts for the use of two rooms for their meetings and a place of deposit for the property of the Society. These arrangements, they have reason to believe, have given general satisfaction to the Members.

The Society published the Third Part of its Proceedings and Memoirs in August last, and has another Part at present passing through the press, the great extent of which has occasioned some delay in its publication. There have been received since last Report, 41 communications from 21 contributors, of which 20 are printed entire in the 3rd and 4th Parts, and full abstracts given in

* Mr. Hennell's paper on the Mutual Action of Sulphuric Acid and Alcohol, reprinted from Phil. Trans., will be found in Phil. Mag., S. 1. vol. lxviii. p. 354.—Edit.

the Proceedings of the remaining 21. These communications are the fruit of numerous and varied inquiries, and form, in the opinion of the Council, a contribution of some importance to the progress of the science. The Council would refer in particular to the full examination and discussion which the process of MM. Will and Varrentrapp for the determination of nitrogen has received by the experiments of Mr. Francis and Dr. Fownes, and more lately in Dr. Will's own comprehensive memoir;—to the series of useful papers on astringent substances, which they owe to their valuable contributor Dr. Stenhouse; and to the papers on various subjects connected with the metals and the salts by Professors Liebig and Gregory, Messrs. Porrett, Croft, Cock, Balmain and Warington, and on organic substances by Professors Liebig, Johnston, Everitt, Drs. Playfair and Fownes; on agricultural subjects by Dr. Schweitzer and Mr. Chatterley; on voltaic electricity by Mr. Arrott, and on the heat disengaged in combinations by the President. The Council still presses upon these and other contributors not to relax their exertions, and invites the Members generally to communicate the results of their inquiries.

The Society has also received presents of interesting chemical products and crystalline specimens for their collection from various donors, particularly Mr. Warington and Professor Liebig. They have also received several chemical works from their respective authors. The Council call attention to this nucleus of a collection which has been formed, and which they hope will be rapidly increased by the exertions and liberality of the Members.

The Council has also lately made arrangements for procuring the leading chemical Journals and circulating them among the Members.

The condition of the Society's finances is highly favourable.

The following gentlemen were elected as Officers and Council for the ensuing year:—

President.—Arthur Aikin, Esq., F.L.S., F.G.S.

Vice-Presidents.—William Thomas Brande, Esq.; John Thomas Cooper, Esq.; Thomas Graham, Esq.; Richard Phillips, Esq.

Treasurer.—Robert Porrett, Esq.

Secretaries.—Robert Warington, Esq., and George Fownes, Ph. D.

Foreign Secretary.—E. F. Teschemacher, Esq.

Council.—Dr. Charles Daubeny; Thomas Everitt, Esq.; Michael Faraday, D.C.L.; J. P. Gassiot, Esq.; Dr. William Gregory; Percival N. Johnson, Esq.; James F. W. Johnston, Esq.; Dr. W. B. Leeson; W. Hallows Miller, Esq.; W. Hasledine Pepys, Esq.; Dr. G. O. Rees; Lieut.-Col. Philip Yorke.

The thanks of the Society were given to the Officers and Council for their exertions during the past year.

April 4.—The following communications were then read:—

76. "On the Subsulphates of Copper," by J. Denham Smith, Esq.

77. "On the Spontaneous Decomposition of the Chlorate of Ammonia," by Mr. Joseph Wonfor.

Having occasion lately to prepare a quantity of this salt, the phæ-

nomena which form the subject of this communication were observed.

The salt was prepared by adding to a saturated boiling solution of bitartrate of ammonia a saturated boiling solution of chlorate of potassa, the liquor being strained from the precipitated cream of tartar and cooled as rapidly as possible, it being observed that the ammoniacal salt underwent a change if allowed to remain at a high temperature for any length of time; the solution was then carefully evaporated at a temperature below 100° Fahr., and again strained from a small portion of cream of tartar which separated as the liquor was concentrated. The chlorate of ammonia crystallizes in small acicular crystals, or in plates similar to the chlorate of potassa. The crystals are very soluble both in water and alcohol, and have a sharp cooling taste.

This salt was partially examined by Vauquelin, but he does not appear to have observed the change it undergoes at the ordinary temperature of the atmosphere, which most likely arose from his using the salt immediately after it was prepared.

In Murray's 'Elements of Chemistry,' vol. ii. p. 544, it is stated that Vauquelin examined this salt: the author remarks, "it crystallizes in fine needles, and appears to be volatile, as there is a considerable loss on evaporating its solution; its taste is extremely sharp; it detonates when placed on a hot body with a red flame; decomposed by heat it gives out chlorine gas, with nitrogen and a little nitrous oxide, hydrochlorate of ammonia with hydrochloric acid remaining."

Brande states, in his 'Elements,' on the authority of Vauquelin (*Ann. de Chim.* xcv. 97), that "this salt probably consists of one proportional of each of its components, or 17 of ammonia + 76 of chloric acid; but its composition has not been experimentally determined." I have analysed the salt, by decomposing it with caustic potash, collecting the ammonia in water acidulated with hydrochloric acid, and evaporating the solution carefully to dryness; the chloric acid was determined by igniting the salt, after the action of potash, in a porcelain capsule; then calculating the amount by the weight of the resulting chloride of potassium, my results gave one equivalent of ammonia, one of chloric acid, and one of water.

After the salt had been prepared a few days, the colour was observed to have changed from white to lemon-yellow, and gave out an odour which powerfully affected the nose when held over the uncorked bottle, irritating the eyes much more than chlorine, and causing a flow of tears; this odour was dissimilar to that of any of the oxides of chlorine. The salt was put away till an opportunity should offer of examining the cause of this change. On going into the laboratory some days after the alteration in the appearance of the salt had been observed, the bottle, which contained about 4 ounces, was found broken into innumerable particles, and the remains of its contents strewed about the floor; on inquiry I was informed that during my absence it had exploded with a loud report. Imagining the explosion was produced by the bottle being closely stoppered, an ounce of the salt was introduced into a *very strong* phial, and con-

nected with a vessel containing a solution of nitrate of silver, through which the products of the decomposition had to pass, the unabsorbed gases being collected in a jar at the pneumatic trough, hoping to collect the gases as they were liberated. After gaseous matter had been quietly evolved for twelve hours, it exploded with greater violence than before, no portion of the bottle remaining (except the neck) larger than a pea. A quantity of chloride of silver had precipitated from the nitrate, and the gas jar contained free nitrogen. Another portion of the salt was then placed on a sand-bath, the temperature of which was about 120° Fahr.; this soon underwent decomposition, but only detonated slightly, giving off dense white fumes, with the smell of nitrous acid.

Finding the salt was so easily decomposed, I proceeded to examine more closely the nature of the changes that took place. 20 grains of the salt were introduced into a strong flask, connected, as in the previous experiment, with a vessel containing solution of nitrate of silver, but with the mercurial instead of the pneumatic trough; the flask was then very carefully warmed by a spirit-lamp; the salt instantly exploded with great violence and a loud detonation, breaking the flask to atoms. Five grains of the salt were then operated upon, without the vessel containing the solution of the silver salt, and the products of the decomposition collected over mercury; they were nitrogen, chlorine, nitrous acid and water, with a little chloride of ammonium; but from the rapidity with which the gases were eliminated, it was impossible to collect the whole of the products of the decomposition, though the experiments were repeated six or seven times, both with and without the vessel containing the solution of nitrate of silver. When five grains of the salt were employed, the tubes (which were filled with mercury when no salt of silver was used) were not broken; still the action was so energetic that it did not allow of accurate indications of the quantity of the gases evolved being obtained.

From the presence of free nitrogen and chlorine, both in the products of the spontaneous and produced decomposition, I am led to conclude that chloride of nitrogen is formed; but as the whole of the products were in no case obtained, it was impossible to determine this experimentally.

XII. *Intelligence and Miscellaneous Articles.*

NEW ANALYSES OF THE CYMOPHANE (CHRYSOBERYL) OF HADDAM. BY M. A. DAMOUR.

THE author states, after reading the analyses of chrysoberyl by M. Awdejew*, he should not have completed the experiments previously commenced, if he had not observed that M. Awdejew's analyses were confined to the chrysoberyls of Brazil and Siberia; but it appeared to him that it would not be useless to examine the composition of the mineral from Haddam, which occurs in well-defined crystals in the midst of the primitive rocks of Connecticut.

Three analyses were performed in the following manner:—In

* S. 3. vol. xxii. p. 501.

order to prevent any admixture of silica by using an agate mortar, the mineral was powdered in a steel mortar. The powder was digested in hot hydrochloric acid and carefully washed, in order to dissolve the iron acquired from the mortar; it was then dried and weighed. It was then fused at a low red heat in a platina crucible with six times its weight of freshly-prepared bisulphate of potash. In half an hour the fusion and decomposition of the mineral were complete; the mass was dissolved in boiling water and filtered; there remained but a few unimportant particles of micaceous or siliceous matter arising from an accidental admixture. The clear solution was saturated with ammonia; the alumina, glucina and oxide of iron were thus precipitated and separated from the bisulphate of potash.

In order to separate the alumina and oxide of iron from the glucina, they were at first dissolved in a slight excess of hydrochloric acid, and the solution was poured into a large quantity of solution of carbonate of ammonia, which precipitated the alumina and oxide of iron and retained the glucina; but by this method only about 12 of every 100 parts of glucina could be obtained.

This process was therefore abandoned, and the method employed by M. Awdejew was adopted; this consists in dissolving the recently precipitated alumina and glucina in a cold solution of potash, the oxide of iron is thus separated; the alkaline solution is then to be diluted, and on boiling it, glucina is precipitated in white flocks, which are easily washed; it dissolves entirely in acids and in excess of carbonate of ammonia; the alumina retained in solution by the potash is precipitated in the usual manner.

The oxide of iron was observed in each operation to carry with it a notable quantity of glucina; in order to separate them they were dissolved in hydrochloric acid; the solution was supersaturated with carbonate of ammonia, and the iron was precipitated with hydrosulphate of ammonia. The glucina was separated by boiling the solution from which the sulphuret of iron had been precipitated; and after washing and heating to redness it was added to that previously obtained.

The mean of three analyses gave as the composition of cymophane,—

Alumina	75·26
Glucina	18·46
Peroxide of iron	4·03
Sand	1·45
	99·20

M. Damour remarks, that not knowing any mode by which to determine directly the degree of oxidizement of iron in minerals not acted upon by acids, he leaves undecided the question as to the mode of its existence in the cymophane, as to whether it is in the state of protoxide, or of peroxide isomorphous with alumina.—*Ann. de Ch. et de Phys.*, Fev. 1843.

ACTION OF NITRIC ACID ON CARBONATE OF LIME. BY M. BARRESWIL.

It is generally admitted by chemists as a fact, that marble is not

acted upon by nitric acid of the greatest density. M. Barreswil, desirous of ascertaining whether this anomaly was due to the same want of action of this acid on certain metals, kept a piece of marble in concentrated nitric acid, and it was not visibly acted upon. It was then removed from the acid, washed, dried and powdered, and the powder was put into fresh concentrated acid; it was strongly acted upon, but not entirely dissolved. A little water was then added to the acid, the reaction again took place, and after some time ceased, but recommenced on the addition of more water. It may be concluded from these facts that marble is attacked by concentrated nitric acid with energy proportional to its surface, becoming covered with a varnish of nitrate of lime insoluble in concentrated nitric acid. This nitrate of lime concentrates the nitric acid in which it is produced, and renders it strong. The experiment performed in a direct mode was perfectly conclusive: dried nitrate of lime, put into nitric acid of moderate strength, concentrated and rendered it fuming.—*Ibid.*

METEOROLOGICAL OBSERVATIONS FOR MAY 1843.

Chiswick.—May 1. Cloudless: cold and dry. 2. Fine. 3. Very fine. 4. Cloudy and fine: rain. 5. Rain: cloudy: constant and very heavy rain at night. 6. Heavy rain: clear and cold at night. 7. Clear and fine: showery: frosty at night. 8. Hazy: heavy rain. 9. Drizzly: cloudy. 10. Slight haze: clear and cold at night. 11. Light haze: clear. 12, 13. Very fine. 14. Cloudy and fine: heavy rain at night. 15, 16. Rain. 17. Heavy showers. 18. Densely overcast: cold rain. 19. Rain: cloudy. 20. Cloudy: showery: heavy rain at night. 21. Fine: heavy rain: clear and cold at night. 22. Heavy showers. 23. Cloudy: lightning with rain at night. 24. Heavy rain: clear. 25. Cloudy and fine. 26. Rain. 27, 28. Showery. 29. Hazy. 30. Light haze: very fine: showery. 31. Cloudy and mild.—Mean temperature of the month 3° below the average. The quantity of rain was greater than that which has fallen in any month within at least the last seventeen years.

Boston.—May 1, 2. Fine. 3. Cloudy. 4. Fine. 5. Cloudy: rain early A.M. 6. Rain: rain A.M. and P.M. 7. Cloudy. 8. Cloudy: rain P.M. 9—11. Cloudy. 12. Fine. 13. Cloudy: rain P.M. 14. Fine. 15. Rain: rain P.M. 16. Cloudy: rain early A.M. 17. Cloudy. 18, 19. Cloudy: rain early A.M. 20. Cloudy. 21. Cloudy: rain early A.M.: rain P.M. 22. Fine. 23. Cloudy: rain A.M. 24. Windy: rain A.M. 25. Fine: rain A.M. 26. Fine. 27. Fine: rain, with thunder and lightning P.M. 28, 29. Fine. 30. Fine: halo round the sun 11 A.M. 31. Cloudy: rain early A.M. This has been the wettest May we have had since 1830.

Sandwick Manse, Orkney.—May 1. Fine: fog. 2. Cloudy: fog. 3. Clear: cloudy. 4. Rain: cloudy. 5. Cloudy: clear. 6. Clear: cloudy. 7. Rain: cloudy. 8. Clear. 9. Clear: cloudy. 10, 11. Clear: fine. 12—14. Cloudy. 15—17. Clear. 18. Cloudy; fine. 19. Cloudy: showers. 20. Bright: clear. 21. Bright: cloudy. 22—24. Bright: clear. 25. Rain. 26. Cloudy. 27. Damp. 28. Cloudy: sleet-showers. 29. Snow-showers: sleet-showers. 30. Bright: fine. 31. Clear: fine.

Applegarth Manse, Dumfries-shire.—May 1—3. Fair and fine. 4. Fair till P.M.: rain. 5. Heavy showers. 6. Fair and fine. 7. A shower. 8, 9. Fair. 10, 11. Fair: hoar-frost. 12. Fine: rain P.M. 13. Fine and mild. 14. Fine, but cloudy. 15. Showers. 16. Cloudy and cold. 17. Cool: cloudy. 18. A shower. 19. Cold. 20. Cold: fair. 21. Cold: wet. 22. Milder, but showery. 23. Mild: cloudy. 24. Cold and rainy. 25. Soft rain. 26. Mild: showers. 27. Mild: showery. 28. Cold and rainy. 29. Clear: heavy rain. 30. Soft: growing: thunder. 31. Wet all day.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary,* Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse,* DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month. 1843. May.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¼ a.m.	Barometer. Orkney, Sandwick. 8½ p.m.	Thermometer. London: R.S. Fahr. 9 a.m.	Thermometer. London: R.S. Self-reg. Mx.	Thermometer. London: R.S. Self-reg. Mn.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumf.-shire. Max.	Thermometer. Dumf.-shire. Min.	Thermometer. Orkney, Sandwick. 9¼ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.
1.	30·130	30·140	30·065	29·40	30·22	30·25	30·50	30·49	59·8	65·0	52·3	70	42	55	67	41	51	44
2.	30·186	30·124	30·004	29·66	30·21	30·08	30·39	30·37	59·0	67·7	49·0	69	40	55·5	65	41	47	43
3.	29·942	29·891	29·778	29·47	29·78	29·76	30·00	29·67	46·0	67·2	43·7	66	44	46	69	38	48	46
4.	29·808	29·768	29·680	29·26	29·60	29·40	29·56	29·55	58·7	62·3	46·3	66	45	54	66	38	47	45
5.	29·720	29·676	29·662	29·09	29·40	29·35	29·53	29·50	56·2	64·8	50·4	68	42	52	52	38	47	43
6.	29·302	29·623	29·258	28·93	29·44	29·36	29·40	29·53	47·4	62·2	48·2	53	32	44	58	42	49	44
☽ 7.	29·676	29·638	29·601	29·11	29·34	29·50	29·40	29·74	52·3	70·6	40·3	59	29	50·5	55½	41½	46	45
8.	29·596	29·579	29·521	29·70	29·62	29·73	29·96	30·07	45·5	60·6	42·3	49	42	51	55	38	48	44
9.	29·642	29·861	29·604	29·22	29·78	29·92	30·12	30·18	47·2	49·5	44·2	54	39	51	58½	42	47½	43
10.	30·044	30·136	29·983	29·54	30·05	30·10	30·25	30·28	50·7	54·7	46·2	57	33	52	61	37	46½	41
11.	30·234	30·184	30·165	29·78	30·11	30·06	30·25	30·22	49·2	56·4	43·8	62	34	47·5	63½	35½	47	42
12.	30·202	30·159	29·879	29·67	30·00	29·74	30·13	29·96	56·7	64·6	46·3	67	46	57	66	37½	45	44
○ 13.	29·948	29·904	29·858	29·21	29·60	29·68	29·82	29·86	56·2	66·2	52·6	66	42	57	55	48	46	46
14.	29·848	29·795	29·552	29·25	29·69	29·53	29·86	29·86	56·7	64·4	48·8	64	50	63	61	42	47	43½
15.	29·544	29·509	29·497	28·90	29·50	29·48	29·82	29·86	55·2	69·0	51·0	65	47	52	49	45	47	43
16.	29·432	29·497	29·386	28·92	29·48	29·30	29·86	29·95	54·8	63·5	50·7	62	50	56	50	45	49½	44
17.	29·518	29·671	29·476	29·05	29·65	29·82	29·97	30·07	52·2	59·3	51·2	52	43	52	56	44	49	43
18.	29·850	29·901	29·798	29·05	29·90	29·96	30·15	30·18	47·7	58·6	45·3	48	45	52	49	40	49	44
19.	29·928	29·893	29·886	29·50	29·95	29·90	30·16	30·15	48·3	52·0	46·3	60	46	51	54	35	50½	44
20.	29·884	29·830	29·641	29·44	29·80	29·78	30·13	30·13	55·0	56·3	49·0	62	48	55	60	43	48½	44
☾ 21.	29·696	29·649	29·637	29·16	29·62	29·60	30·01	30·08	56·3	59·3	50·8	64	39	54·5	53	43	51	48
22.	29·718	29·667	29·653	29·10	29·62	29·63	30·03	30·03	56·8	65·3	47·8	56	45	60	56	42	50	47
23.	29·750	29·784	29·616	29·16	29·65	29·65	30·03	30·05	57·5	62·2	51·6	68	52	54	56½	45	53¾	47
24.	29·532	29·553	29·479	29·02	29·62	29·55	30·05	30·00	54·2	62·6	53·8	68	44	56	51	41	52	48
25.	29·676	29·677	29·605	29·03	29·52	29·45	29·80	29·52	59·8	73·4	51·0	67	41	62	66	45	48½	48
26.	29·754	29·697	29·457	29·06	29·40	29·32	29·49	29·42	60·2	68·2	50·6	66	45	60	56	...	52	49
27.	29·570	29·535	29·433	28·90	......	29·10	29·42	29·54	59·8	65·7	50·3	63	43	59·5	61	...	53½	41½
28.	29·560	29·802	29·520	28·96	......	29·70	29·70	29·82	56·7	63·0	47·8	65	44	58	53	40	45½	39
● 29.	29·966	30·032	29·912	29·40	29·80	29·90	29·83	29·92	48·2	62·2	46·2	54	34	54	57½	42	39	40
30.	30·144	30·084	29·958	29·54	29·90	29·82	29·98	29·97	54·3	61·7	42·3	64	48	57	56	45	40	43
31.	29·906	29·854	29·813	29·26	29·60	29·60	29·80	29·82	59·3	64·6	51·7	67	54	59	54	44	47	42
Mean.	29·800	29·810	29·689	29·25	29·722	29·683	29·918	29·938	54·1	64·3	48·2	51·64	42·84	54·4	57·7	41·3	47·98	44·19

Days of Month. 1843. May.	Wind. London: R.S. 9 a.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. London: R.S. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.	Dew-point. Lond. R.S. 9 a.m.
1.	e.	e.	e.	ene.	wnw.	......	......	......	......	......	49
2.	e.	e.	e.	ne.	se.	......	......	......	......	......	51
3.	e.	ne.	e.	ne.	w.	......	·06	......	......	......	45
4.	e.	sw.	calm	nw.	nw.	·022	·16	......	......	·17	50
5.	s.	sw.	calm	w.	e.	·158	1·26	·04	......	·09	52
6.	nnw.	w.	calm	w.	s.	1·133	·38	·40	......	......	51
☽ 7.	s.	s.	calm	s.	e.	·300	·65	·63	......	·55	45
8.	nne.	e.	e.	e.	e.	·036	·57	......	0·22	......	45
9.	n.	ne.	e.	ne.	e.	·463	·02	·06	......	......	45
10.	n.	e.	calm	ese.	e.	......	......	......	......	......	46
11.	n.	ne.	calm	s.	se.	......	......	......	......	......	45
12.	s.	sw.	calm	s.	ese.	......	·02	......	......	......	47
○ 13.	s.	w.	calm	s.	ese.	·033	......	......	......	......	51
14.	s.	s.	calm	ne.	e.	......	·16	·06	......	......	50
15.	s.	sw.	calm	se.	e.	·219	·14	·48	......	......	53
16.	e.	ne.	calm	se.	e.	·100	·07	·17	0·30	......	51
17.	w.	ne.	calm	s.	e.	·063	·20	......	......	......	52
18.	n.	ne.	calm	se.	nw.	·283	·04	·58	......	......	46
19.	e.	ne.	e.	e.	ne.	·080	·02	·09	......	......	48
20.	e.	e.	e.	se.	e.	......	·44	......	......	·07	48
☾ 21.	s.	s.	calm	se.	e.	·302	·05	·39	......	......	52
22.	sse.	sw.	calm	se.	ne.	·063	·09	·41	......	......	51
23.	sse.	se.	calm	se.	e.	·086	·53	......	......	......	52
24.	e.	s.	e.	e.	e.	·380	·06	·09	0·10	......	54
25.	s.	sw.	calm	s.	e.	......	·03	·12	......	......	53
26.	s.	sw.	w.	s.	se.	......	·20	·03	......	1·12	55
27.	se.	sw.	calm	se.	ne.	·036	·14	......	......	......	55
28.	s.	w.	w.	sw.	n.	·150	·16	·20	......	......	52
● 29.	e.	e.	calm	w.	nw.	·211	·30	......	......	·27	46
30.	e.	s.	calm	w.	nw.	·175	·03	......	1·50	·04	49
31.	sw var.	w.	calm	e.	nw.	·016	·06	·05	......	......	54
Mean.						Sum. 4·309	5·25	3·00	2·12	2·31	Mean. 50

THE

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[THIRD SERIES.]

AUGUST 1843.

XIII. *Examination of the Cowdie Pine Resin.* By Robert D. Thomson, *M.D., Conductor of the Laboratory and of the Classes of Practical Chemistry in the University of Glasgow**.

THE Cowdie resin has been known for some years to those botanists who are familiar with the vegetation of New Zealand. Mr. Robert Brown informs me that he possesses a very large and elegant specimen of this substance; but it does not appear to have hitherto attracted the attention of chemists. I have been acquainted with its external properties for some years, from a specimen in our private chemical museum in the College, but it was only in the course of last spring that my attention was particularly called to its examination, in consequence of having large and beautiful specimens presented to me by my friend Dr. Ernst Dieffenbach, formerly of Giessen and lately naturalist to the New Zealand Company, who, by his laborious and indefatigable exertions while resident in New Zealand, has contributed so extensively to our knowledge of the moral and physical condition of that interesting British colony†.

I am indebted to Mr. Robert Brown for the information that this resin is derived from the *Dammara Australis*, a tree which belongs to the natural order *Coniferæ*, and division *Abietinæ*. (See also Lambert's Pines.) The resin is, I believe, known by the native name of "*Cowdie*," and in consequence the tree from which it exudes is usually termed the "Cowdie Pine." There is an excellent specimen of this pine in the Glasgow Botanic Garden, on which I have been able to detect distinct traces of a resinous exudation. In the same

* Read before the Philosophical Society of Glasgow, March 15, 1843; and now communicated by the Author. A paper on the Cowdie Resin, by Mr. Prideaux, will be found in Phil. Mag. S. iii. vol. xii. p. 249.

† See Dieffenbach's Travels in New Zealand. London, 8vo. 2 vols.

garden, also, there is a specimen of the *Dammara orientalis*, from which the dammara resin previously described by chemical writers is probably derived*; and on the stem of this species I have also observed unequivocal proofs of the presence of a resin. The cowdie resin occurs in large masses, from the size of the fist to a much greater magnitude. It is transparent when freshly fractured; but as it comes from New Zealand, generally it is slightly opalescent, a character which is said to be produced by the action of water or moisture. The colour of the resin is light amber. It is easily fused, and then emits a resinous or turpentine odour. A small portion of the resin dissolves in weak alcohol, but the greater part remains insoluble. The solution in alcohol evolves the smell of turpentine. The resin, when agitated with hot absolute alcohol, forms a fine varnish. A similar result follows its treatment with oil of turpentine, which might be found valuable in the arts; sulphuric acid dissolves it; and water added to the solution precipitates the resin in flocks.

Resins are usually divided into two classes, and are termed, according to their characters, *acid* and *neutral* resins. The cowdie resin appears to belong to both of these classes. When boiled with spirit a portion of the resin dissolves, and there remains a white resin, which is insoluble in weaker spirit, but which forms with absolute alcohol a fine transparent varnish. That portion of the resin which dissolves in weak alcohol possesses all the qualities of an acid, forming salts with metallic oxides, and is not precipitated by ammonia, while the precipitate occasioned by adding water to the alcholic solution is quite soluble in ammonia.

The alcoholic solution of the acid portion of the resin reddens vegetable blues. I propose to term it *Dammaric* acid; while the residual white resin may be called *Dammaran*, to distinguish it from the Dammarin of Lecanu and Brandes.

Entire Resin.

The entire resin without the action of any chemical reagent was pulverized and dried at 212°, and afforded in two analyses the following results:—

I. 9·435 grs. gave 25·71 CO_2 and 8·73 HO
II. 5·69 ... ... 15·565 CO_2

Hence we have	I.	II.	Mean.
Carbon . . .	74·30	74·60	74·45
Hydrogen .	10·28		10·28
Oxygen. . .	15·42		15·27
	100·		100·

* Lecanu and Brandes, Thomson's Veget. Chem., p. 538.

To determine whether the resin was sufficiently dried, a portion was fused and exposed to a temperature of 350° for some time. The following were the results of two analyses:—

I. 6·97 grs. gave 19·30 grs. CO_2 and 6·18 HO
II. 7·96 ... ... 6·93 HO

This is equivalent to

	I.	II.	Mean.
Carbon . . .	75·46		75·46
Hydrogen .	9·85	9·67	9·76
Oxygen . . .	14·69		14·78
	100·		100·

From these data we may deduce the following composition:—

			Calculation.	Experiment.
Carbon . .	·75*	× 40 = 30·	=75·23	75·46
Hydrogen	·125	× 31 = 3·875	9·73	9·76
Oxygen . .	1·	× 6 = 6·	15·04	14·78
		39·875	100·	100·

The close correspondence of the theoretical and practical results, in reference more especially to the hydrogen, may lead us with some degree of confidence to assume the following formula as representing the composition of the cowdie resin:—

$$C_{40}\,H_{31}\,O_6;$$

and adopting an analogous view to that of Liebig, in reference to the composition of turpentine resins, we may consider the basis of the resin—

$$C_{40}\,H_{30}\,{H \atop H}$$

becoming, by the substitution of one atom of oxygen for one of hydrogen, and the addition of oxygen,

$$C_{40}\,H_{30}\,{H \atop O}+O_5.$$

Hydrous Dammaric Acid.

The resin was boiled in successive portions of alcohol until it ceased to lose more of its substance. The solution was then precipitated by water. The precipitated resin was washed and dried at 212°, but not fused.

* Dr. Thomson first deduced ·75 for the atom of carbon (in 1813) from the specific gravity of carbonic acid, and from his analysis of olefiant gas. In the same year he fixed ·878 as the atom of azote. Now this number is almost exactly half of his present number, for ·878 × 2 = 1·756. It may not be out of place to mention, that in his recent works on organic chemistry he has recalculated all the formulæ of the foreign chemists by his own atomic weights, which have been recently so strikingly confirmed by Dumas and others.

6·9 grs. gave when burned with oxide of copper, 18·39 CO_2 and 5·78 HO.

The composition of the hydrous acid is therefore—

	Experiment.	Calculation.	Atoms.
Carbon . . .	72·69	73·39	40
Hydrogen . .	9·31	9·47	31
Oxygen . . .	18·00	17·14	7
	100·	100·	

This approaches the formula

$$C_{40}\,H_{31}\,O_7.$$

If the alcoholic solution of the dammaric acid be allowed to evaporate spontaneously, the resin is deposited apparently in the form of crystalline grains.

Anhydrous Dammaric Acid.

To determine the atomic weight, a solution of dammaric acid in alcohol was mixed at a boiling temperature with an alcoholic solution of nitrate of silver, to which some caustic ammonia had been added. The silver salt after being washed and dried was analysed.

4·26 grs. gave by ignition ·58 silver = ·622 oxide of silver. From which we have—

Oxide of silver .	14·60		14·75	1 atom.
Dammaric acid .	85·40	=	86·27	2 atoms.
	100·00		101·02	

To determine the composition of the anhydrous acid, the silver salt was analysed.

6·62 grs. gave when burned with oxide of copper 15·73 CO_2 and 5·39 HO.

The composition of the silver salt is therefore,—

			Atomic weight.
Carbon	64·78	65·45	86·27, or
Hydrogen	9·01	9·11	43·13 × 2.
Oxygen	11·61	11·72	
Oxide of silver . .	14·30	14·75	
	100·	101·02	

And that of the anhydrous acid is—

	Experiment.		Calculation.
Carbon . . .	75·85	43 × ·75 = 32·25	75·43
Hydrogen .	10·56 =	36 × ·125 = 4·5	10·52
Oxygen . .	13·59	6 × 6· = 6·	14·05
	100·00	42·75	100·

Hence the formula of the anhydrous acid corresponds with

$$C_{43}\,H_{36}\,O_6,$$

and the silver salt is

Bidammarate of silver 2 (C_{43} H_{36} O_6) + AgO:

or supposing the hydrogen in excess, which is generally the case with a resin unless it has been fused, the anhydrous acid would be as follows:—

Carbon	. . 40 × ·75	= 30·	75·47
Hydrogen	. 30 × ·125	= 3·75	9·43
Oxygen	. . 1 × 6·	= 6·	15·09
		39·75	

The difference between the hydrous and anhydrous acids would then be one atom of water.

Hydrous acid . . . . C_{40} H_{31} O_7
Anhydrous acid . . . C_{40} H_{30} O_6
H O

Dammaran.

I give this name to the substance remaining after the separation of the dammaric acid. It is a fine white brittle resin, apparently insoluble in weaker spirit, but forming with absolute alcohol a beautiful colourless varnish, and also a similar preparation with oil of turpentine. This substance is identical in composition with the resin. When dried at 212° its composition was as follows:—

7·4 grs. burned with oxide of copper gave 20·36 CO_2 and 6·4 HO.

This is equivalent to

Carbon.	75·02
Hydrogen	9·60
Oxygen	15·38
	100·00

This approaches closely

C_{40} H_{31} O_6.

By exposing this substance to a higher and longer continued heat, it was found to absorb oxygen and to alter in its composition, as appears by the following analyses:—

I. 6·57 grs. gave 16·75 CO_2 and 5·7 HO.
II. 7·64 ... 20·33 CO_2 and 6·7 HO.

	I.	II.
Carbon	72·56	69·25
Hydrogen . . .	9·74	10·32
Oxygen	17·70	20·43
	100·	100·

The first specimen was dried at 300° for three days; the second at 350° for four days.

The influence of heat may hence account in some degree for the more rapid formation of resins from oils of the turpentine type in warm countries, and also for the greater solidity which resins acquire than in the more temperate latitudes.

Dammarol.

When the dammara resin is exposed to a carefully regulated temperature it melts, and heavy vapours arise, which gradually and slowly pass over and condense in the form of a fine amber-coloured oil swimming on the surface of water; hence by this treatment the resin is resolved into an oil, which may be termed dammarol, and water. By evaporation at 300° the water is dissipated and the oil remains. It boils at a more elevated temperature than water. After rectification, 5·98 grs. burned with oxide of copper gave

$$18{\cdot}03 \text{ grs. } CO_2 \text{ and } 6{\cdot}02\ HO,$$

which makes the composition of dammarol,—

Carbon	82·22
Hydrogen	11·14
Oxygen	6·64
	100·

This approximates

$$C_{40}\, H_{28}\, O_3,$$

and when compared with dammaran—

$$\begin{array}{lll} \text{Dammaran} & \ldots & C_{40}\, H_{31}\, O_6 \\ \text{Dammarol} & \ldots & \underline{C_{40}\, H_{28}\, O_3} \\ & & H_3\ \ O_3 = 3\, HO \end{array}$$

shows the removal of 3 atoms of water. The analysis gives an excess of hydrogen from the retention of some water.

The action of heat upon resins was known as early as 1688 (*Mémoires de l'Académie Royale des Sciences de Paris*, 1688), and the relative proportions of water and oil obtained by the distillation of these bodies was accurately noted. Colophon or common resin, for example, it is stated, when distilled in the quantity of 2 pounds, afforded 26 ounces 4 drachms of oil, and 3 ounces 1 dr. of an acid liquid. Neumann, a most sagacious chemical writer, whose works may even yet be consulted with benefit by modern chemists, was well aware of the nature of resin. "Essential oils," he says, "by digestion or heat (Neumann's Chemistry, by Lewis, 4to. 1758, p. 269) change into balsams, and at length into brittle resins. Distilled again in this state they yield, like most of the natural resins, a portion of fluid-oil."

The effect of heat in removing water from resin, as now stated, enables us to explain the process for preparing copal

varnish. Copal in its natural state is insoluble in oil of turpentine; but when fused and boiled until water is removed, the residual oil (*Copalol?*) is soluble in that menstruum. The nature of the product from the distillation of resins depends on the temperature. From a mixture of oils procured by distilling common resin at a high temperature I have obtained a considerable proportion of creasote.

Dammarone.

When dammara resin is finely pounded and mixed intimately with 5 or 6 times its weight of quicklime, and the united powders are distilled by the heat of a spirit-lamp carefully, either in a tube retort or in a larger vessel, if the quantity experimented on is more considerable, dense white fumes speedily make their appearance, which condense in the receiver first in the form of water, having an æthereal odour, and gradually as a thick amber-coloured oil, which floats on the surface of the water. By the application of heat the water soon disappears, while a dark oil remains, which may be further purified by rectification. This oil is exceedingly liquid when hot, but on cooling and exposure to the air it becomes thicker. Its boiling point is above 270° F. It burns with a dense smoke, and is soluble in alcohol.

4·3 grs. gave when burned with oxide of copper 13·59 CO_2 and 4·465 grs. HO.

This is equivalent to

Carbon	86·22
Hydrogen	11·53
Oxygen	2·25
	100·

This corresponds nearly with

Carbon	38	× ·75	= 28·5		85·64
Hydrogen . .	30	× ·125	= 3·75		11·27
Oxygen	1	× 1·	= 1·		3·09
			33·25		100·00

The comparative formula will then be

Dammaran . . $C_{40}\,H_{31}\,O_6$

Dammarone . . $C_{38}\,H_{30}\,O$

$$2\quad 1\quad 5 = 2\,CO_2 + HO.$$

The experimental result gives a larger amount of carbon, which I believe to be owing to the difficulty of separating the whole of the carbo-hydrogen oil which forms the basis of the resin, and is disengaged in the first stage of the distillation. All those who are familiar with this branch of chemistry are aware of this obstacle to precise formulæ. The experiment

shows that two atoms of carbonic acid and one of water have been removed by the action of the lime. That carbonic acid is fixed by the lime, is proved by the effervescence which takes place when acid is poured on the residue in the retort.

Dr. French of London, in his 'Art of Distillation,' published in 1664, was aware that by means of lime, oils might be extracted from "*resins, gums, fat and oily things.*" To obtain them, he directs 1 lb. of any of these to be distilled with 3 lbs. of the powder of tiles or unslaked lime. I have not been able to find any notice of these facts in Glauber or any anterior writer.

The preceding experiments assist in carrying out certain generalizations which had been deduced from a limited series of data, and serve to confirm the idea of the analogy of the resins, and of their derivation from an oil of the turpentine type. The resins perhaps are more interesting to the chemist than at first appears, from their analogy to other bodies of vegetable and animal origin. Whether their basic oils are derived from the deoxidation of other bodies in plants supplied with a larger amount of oxygen, or are formed directly from their gaseous constituents, is a subject for inquiry. If it be true that plants evolve no heat (although it is not easy to comprehend how gases can be condensed without such a disengagement), then it would appear that no combination of carbon and oxygen, no proper combustion, such as occurs in the animal system, takes place in plants; and hence it would follow that the essential oils are formed directly from their elementary constituents. But the statement (Brongniart) which has been made that plants evolve heat in fertilization, that oxygen is absorbed and carbonic acid given out, would appear to favour the idea that combustion can occur in plants as well as in animals. The admission of the operation of this process in plants, would throw much light on the following table, representing a descending series, with the exception of the first, into which some bodies of animal origin are introduced for the sake of comparison.

Protein	$C_{48}\ H_{36}\ O_{14}\ N_6$
Gum	$C_{48}\ H_{44}\ O_{44}$
Starch	$C_{48}\ H_{40}\ O_{40}$
Base of cane-sugar	$C_{48}\ H_{36}\ O_{36}$
Fat	$C_{44}\ H_{40}\ O_4$
Bees-wax	$C_{40}\ H_{40}\ O_2$
Dammaran	$C_{40}\ H_{31}\ O_6$
Cholesterin	$C_{38}\ H_{32}\ O$
Dammarone	$C_{38}\ H_{30}\ O$
Base of resins	$C_{40}\ H_{32}$

In reference to the preceding table, the analogies of starch, gum and sugar are sufficiently familiar, both in the artificial processes, by which the former may be transformed into the latter, and in the changes produced by vegetation. The conversion of sugar and honey into wax by bees was long ago shown by Huber, and has lately been brought forward with happy effect by Liebig, in evidence of the part which the saccharine class of bodies performs in the respiratory œconomy. The intermediate position which fat holds between sugar and wax would seem to point to it as a stage in the process of reduction. The analogy between wax and cholesterin is sufficiently striking as products of reduction from an amylaceous or saccharine base; and this idea has been strengthened by the circumstance of my having obtained from the latter bodies bearing a close analogy to the turpentine and naphtha type, while the opinion has gained support, which I entertain, that cholesterin is the wax of mammiferous animals. The correspondence of resins to these bodies is sufficiently apparent.

XIV. *Reply to* Mr. Cayley's *Remarks.* *By the Rev.* Brice Bronwin*.

A DESIRE to see the paper which I last transmitted to this Journal printed before my reply to Mr. Cayley, has occasioned this delay in noticing his remarks of the 13th of April ult. (inserted in the Number for May, p. 358) on a former paper of mine. With respect to the second form of ω, he says, that I by no means show that Jacobi's formulæ fail, but rather confirm them. Now I did not say that they failed for it, but only that they were reducible till it disappeared, and that with it their second members were improper representations of the first. And if this be correct, which Mr. Cayley does not deny, it is surely to be discarded from the theory.

For the other three forms, Mr. Cayley thinks that when $u = \omega$, $\frac{u}{\mathrm{M}}$ might be $p\,\mathrm{H} + p'\,\mathrm{H}'\sqrt{-1}$, and $\mathrm{s}\,a\,v = \pm 1,\ 0,\ \pm\infty\sqrt{-1}$, or $\pm\frac{1}{\lambda}$. I presume he means $\frac{1}{\lambda}$, not $\frac{1}{k}$. I consider that the structure of the formulæ implies that $\mathrm{s}\,a\,\frac{u}{\mathrm{M}}$, $\mathrm{c}\,a\,\frac{u}{\mathrm{M}}$ do not exceed the limits ± 1, and therefore reject

* Communicated by the Author.

$\pm \infty \sqrt{-1}$ and $\pm \frac{1}{\lambda}$. And as 0 relates to the second form of ω, it is to be rejected. There is certainly room for discussion as to whether the quantities p and p' are to be determined or assumed. I assumed them, and took the least values, because it did not affect my conclusions. Were I to discuss the various points to which this difference between me and Mr. Cayley gives rise, I should extend this paper to too great a length. And as I think I can place the subject in a clearer light by a much shorter process, I prefer doing so.

But first I must beg to call Mr. Cayley's attention to a real transformation at page 54 of Jacobi's work. It is derived by the aid of imaginary quantities, and from an $\omega = \frac{K' \sqrt{-1}}{n}$, and is therefore of the third form. Will Mr. Cayley be prevailed upon to make trial of it in its simplest case, or when $n = 3$, and see if he find it to be a transformation? It is but right to say that I have done so, and did not make it to be one. And if I am correct, this must be fatal to the third form of ω.

And I must observe, that though Jacobi has shown the possibility of such a transformation as he has given, by showing that there are sufficient equations to determine the constants, he has not shown that any and every value of ω will give one. Suffice it that there is one value, or a series of values, namely those included in the first form of this quantity. Nor has he assigned any reasons for the different forms of it which he has suggested. Moreover, he has set out from an assumed equation $1 - y = f(x)$, page 39, from which all the rest of the formulæ are derived. In this assumed equation he has not actually determined the constants, but only assumed them. If they were actually determined, it might appear that they are not susceptible of that generality which their author and Mr. Cayley suppose.

M. Jacobi's formulæ, as Mr. Cayley has reduced them, are

$$s\,a\,v = \frac{s\,a\,u\,s\,a\,(u + 2\,\omega) \ldots\ldots\ldots s\,a\,(u + 2\,(n-1)\,\omega)}{s\,a\,(K - 2\,\omega)\,s\,a(K - 4\,\omega) \ldots s\,a\,(K - 2\,(n-1)\,\omega)} \ldots (1.)$$

$$c\,a\,v = \frac{c\,a\,u\,c\,a\,(u + 2\,\omega) \ldots\ldots\ldots c\,a\,(u + 2\,(n-1)\,\omega)}{c\,a\,2\,\omega\,c\,a\,4\,\omega \ldots\ldots c\,a\,2\,(n-1)\,\omega} \ldots (2.)$$

The numerator of (1.) when developed is

$$s\,a\,u\,(s^2\,a\,2\,\omega - s^2\,a\,u) \ldots\ldots\ldots$$

and that of (2.) is

$$c\,a\,u\,(s^2\,a\,(K - 2\,\omega) - s^2\,a\,u) \ldots\ldots$$

These formulæ, therefore, by suitable values of u, are constructed to fulfil the conditions $\mathrm{s}\,a\,v = 0$, $\mathrm{c}\,a\,v = \pm 1$, and also $\mathrm{s}\,a\,v = \pm 1$, $\mathrm{c}\,a\,v = 0$. And it must be possible to satisfy them both. For at page 40, in deriving the value of $y = \mathrm{s}\,a\,v$ from that of $1 - y$, Jacobi finds $y = 0$ when $u = 0$, $2\,\omega$, &c. And at page 41, in finding the value of M, he makes $x = \mathrm{s}\,a\,u = 1$, $y = 1$. Also the values of λ and of $1 \pm \lambda\,y$ depend on those of M and of y. Both these conditions therefore are at the very foundation of Jacobi's theory.

He also makes u, $u + 2\,\omega$, &c., and even 0, $2\,\omega$, $4\,\omega$, &c., successive values of u. This decides the form of u. If the general form of ω be $\dfrac{p\,\mathrm{K} + p'\,\mathrm{K}'\sqrt{-1}}{n}$, that of u is $\dfrac{p\,\theta + p'\,\theta'\sqrt{-1}}{n}$; θ and θ' being real elliptic functions, having the common amplitude η, and the moduli k and k' respectively. When $\eta = 0, \frac{\pi}{2}, \pi$, &c., $\theta = 0$, K, 2 K, &c.; $\theta' = 0$, K′, 2 K′, &c.; and $u = 0$, ω, $2\,\omega$, &c. For the three forms of ω which we have to consider these values of u fulfil the condition $\mathrm{s}\,a\,v = 0$, $\mathrm{c}\,a\,v = \pm 1$. For the first form of ω, when the denominator of (1.) reduces to $\mathrm{s}\,a\,\omega\,\mathrm{s}\,a\,3\,\omega \,\ldots\ldots$, the values $u = 0$, $3\,\omega$, &c. satisfy the condition $\mathrm{s}\,a\,v = \pm 1$, $\mathrm{c}\,a\,v = 0$ also. But for the third and fourth forms this denominator cannot be so reduced, nor can u be made to take any of the forms $\mathrm{K} - 2\,\omega$, $\mathrm{K} - 4\,\omega$, &c., or $2\,\omega - \mathrm{K}$, $4\,\omega - \mathrm{K}$, &c. For it could only assume them when η has some of the values $\frac{\pi}{2}$, π, &c., and consequently θ and θ' some of the corresponding values K, K′, 2 K, 2 K′, &c. But for none of these values will u become any one of the quantities $2\,r\,\omega - \mathrm{K}$. The third and fourth forms of ω therefore will not fulfil the conditions to which the formulæ have been subjected, and consequently they must be rejected.

We might take a shorter course. It is sufficient to observe that the first form of ω only will satisfy the conditions $\mathrm{s}\,a\,u = 0$ and $\mathrm{s}\,a\,u = 1$ required by Jacobi's theory, pages 40 and 41. Mr. Cayley says, I have brought no objection against any particular step of Jacobi's reasoning. I suppose them to be all quite correct. But any assumed form of ω will not necessarily fulfil all the required conditions. It must be remembered that these forms are assumed, not determined.

B. Bronwin.

Gunthwaite Hall, June 15, 1843.

XV. *Additional Objections to* Redfield's *Theory of Storms. By* Robert Hare, *M.D., Professor of Chemistry in the University of Pennsylvania**.

50. IN a communication to the London and Edinburgh Magazine and Journal of Science for December 1841, I endeavoured to point out various errors and inconsistencies in the theory of storms proposed by Mr. Redfield, or in the reasoning and assumed scientific principles on which that theory had been advanced. Of these errors I will present a brief summary.

51. I conceive that Mr. Redfield has erred in ascribing atmospheric currents, whether constituting trade winds or storms of any kind, " solely to mechanical gravitation as connected with the rotatory and orbitual motion of the earth †."

52. In ascribing those atmospheric gyrations, of which according to his hypothesis all storms consist, to " opposing and unequal forces," without specifying the nature or accounting for the existence of these forces, although implying that they originate as above mentioned.

53. In assigning to all fluid matter a tendency to " run into whirls and circuits, when subjected to opposing and unequal forces," when this allegation, if true at all, can only be so in some peculiar cases of such forces.

54. In alleging all storms to be whirlwinds, and yet representing a " rotative movement in air as the only cause of destructive winds and tempests," so that a whirl is the only cause of its own violence ‡.

55. In averring, in reference to the alleged gyration and vortical force of tornadoes which are by him treated as hurricanes in miniature, that " all narrow and violent vortices have a spiral involute motion quickening in its gyration as it approaches the centre or axis of the whirl," whereas it must be evident that when gyration in a fluid does not result from a contemporaneous centripetal force, arising from an ascending or descending current at the axis, but on the contrary exists only in consequence of a momentum previously acquired, the consequent velocity in any part of the mass affected, will be less in proportion to its proximity to the axis: also that the only case in which it can increase with its proximity, is where the mass is fluid and it proceeds from some competent cause acting at the axis.

56. In representing that the upward force of tornadoes

* Communicated by the Author. Mr. Redfield's papers will be found in Phil. Mag. S. 3. vol. xx. p. 353; vol. xxii. p. 38.

† See paragraph 60 of this essay.

‡ Silliman's Journal, vol. xxi. p. 192: " Storms and hurricanes consist in the regular gyratory motion or action of a progressive body of atmosphere, which action is the sole cause of the violence which they may exhibit."

is the effect of a vortical or gyratory action *, when it must be quite plain that a "vortical" action or whirling motion instead of causing the air upon the terrestrial surface, necessarily subjected by it to a centrifugal force, to seek the centre, would induce that portion of the atmosphere which should be above the sphere of the gyration, to descend into the central space rarefied by the centrifugal force.

57. In admitting the gyration, which he considers as the cause of storms, to quicken as it approaches the axis of motion, without perceiving that this characteristic is irreconcilable with his inference that gyration caused by forces acting remotely from the axis is the proximate cause of all the phænomena in question.

58. In the number of Silliman's Journal of Science for April, 1842, Mr. Redfield has hinted that the pains which I have taken to confute his doctrines are disproportioned to the low estimation in which I have professed to hold them. I should be glad if this view of the subject should render my strictures agreeable to him; and am sincerely sorry that, consistently with truth, I cannot directly take a course more favourable to his meteorological infallibility. I admit that his essays have met with an attention which may have justified him in pluming himself on their success. Had it been otherwise, I should not have thought it worth while to enter the lists. It strikes me, however, that a fault now prevails which is the opposite of that which Bacon has been applauded for correcting. Instead of the extreme of entertaining plausible theories having no adequate foundation in observation or experiment, some men of science of the present time are prone to lend a favourable ear to any hypothesis, however in itself absurd, provided it be associated with observations. But to proceed with the "reply," so called, the author alleges that in the absence of "reliable facts and observations" in support of my objections to what he considers as the "established character of storms," he had hesitated to answer them. This cannot excite surprise, when it is recollected "that the whole modern meteorological school," and likewise "Sir John Herschel," are accused by him of a "grand error," in not ascribing all atmospheric winds "solely to the gravitating power as connected with the rotary and orbitual motion of the earth."

59. For this denunciation he has no better ground than that on which he deems his theory to be above my reach, that is to say, because himself and others have made some observations showing that in certain storms, agreeably to log-book records, certain ships have had the wind in a way to indicate

* See paragraph 92 of this communication.

gyration. Being under the impression, that in many instances no better answer need be given to Mr. Redfield's opinions than that created in the minds of scientific readers by his own language, I will here quote his denunciation of the opinions of the meteorological school and of Herschel.

60. "The grand error into which the whole school of meteorologists appear to have fallen, consists in ascribing to heat and rarefaction the origin and support of the great atmospheric currents which are found to prevail over a great portion of the globe. * * * An adequate and undeniable cause for the production of the phænomena * * I consider is furnished in the rotative motion of the earth upon its axis, in which originate the centrifugal and other modifying influences of the gravitating power, which must always operate upon the great oceans of fluid and aërial matter, which rest upon the earth's crust, producing of necessity those great currents to which we have alluded."—(See Silliman's Journal, vol. xxviii. p. 316.) Speaking of Sir John Herschel's explanation of the trade winds and others, Mr. Redfield alleges, "Sir John has however erred, like his predecessors, in ascribing mainly, if not primarily, to heat and rarefaction those results which should have been ascribed solely to mechanical gravitation as connected with the rotative and orbitual motion of the earth's surface."

61. Is it not surprising that it did not occur to the author of these remarks, that an astronomer so eminent as Sir John Herschel would be less likely than himself to be ignorant of any atmospheric influence resulting from gravitation or the diurnal and annual revolutions of our planet—and that when he found himself in opposition to the whole school of meteorologists, a doubt did not arise whether the "grand error" was not in his views of the subject instead of that which they had taken?

62. It seems to have been forgotten, that all the aqueous portion of the terrestrial surface being, no less than the superincumbent atmosphere, subjected to the gravitating power and the rotary and orbitual motions of our planet, no impulse can be given to the one which is not received by the other; and that as the heavier the fluid the greater the influence, if this be competent to create gales in the atmosphere, it must be no less competent to produce torrents in the ocean. Moreover, do not his opinions conflict not only with the whole school of meteorologists, but also with a portion of the modern school of geology? Agreeably to the last-mentioned school, the external portion of the earth consists of a comparatively thin shell of earth and water floating upon an ocean of matter kept in fusion by heat; the oblate spheroidal form of our planet being due to the perfect equilibrium of the "gravitating, ro-

tary, and orbital" forces which are most inconsistently represented by Mr. Redfield as having upon the atmosphere an opposite effect.

63. But notwithstanding the opinions expressed in the paragraphs above quoted, and in the following, Mr. Redfield alleges in his reply to my objections, that it is an error to consider him as rejecting the influence of heat. It is very possible that his opinions may have changed since he read my "objections;" but that he did reject the influence of heat when the preceding and following opinions were published must be quite evident. "Were it possible to preserve the atmosphere in a uniform temperature all over the surface of the globe, the general winds would not be less brisk than at present, but would be more constant and uniform than ever."—(Silliman's Journal, vol. xxviii. p. 318.)

64. Mr. Redfield alleges that the proper inquiry is, What are storms? not How are storms produced? And yet it will be found that his great object has been to show that they arise from gyration caused by unequal forces generated in some inexplicable mode by gravitation and the complicated motions of our planet. But suppose that before ascertaining how fire is produced, chemists had waited for an answer to the question what is fire, how much had science been retarded! I do not therefore blame Mr. Redfield for pursuing both inquiries simultaneously, inconsistently with his own rule above stated, but I am astonished that he should, without any new experiments or any demonstrations, by an *ipse dixit* undertake to make a novel application of the gravitating power, and the forces arising from the earth's motion; and to inform one of the most eminent astronomers of the age that he had committed an error in overlooking their all-important meteorological influence.

65. Turning from an endless controversy with a writer with whom I differ respecting first principles, I shall address myself to that great school of meteorologists who concur with me in the "grand error" of considering heat and electricity as the principal agents of nature in the production of storms, and who do not concur with Mr. Redfield in considering gravitation and the earth's annual and diurnal motion as the great destroyer of atmospheric equilibrium. So far as it may conduce to truth, I shall incidentally notice some parts of Mr. Redfield's reply; but my main object will be to show the inconsistency of his theoretic inferences with the laws of nature, and the facts and observations on which those inferences are alleged to be founded. To follow him in detail through all the misunderstandings which have arisen, and which would inevi-

tably arise during a continued controversy, would be an Ixion task.

66. Speaking of the trade winds and monsoons, our author states, " It is to the operation and effect of these great and regular moving masses, that we are disposed mainly to ascribe the more active and striking meteorological phænomena of every latitude." And again, " At these seasons the northern margin or parallels of the trade winds sweeping towards the gulf, must necessarily come in collision with the great archipelago of islands which skirt the Carribean Sea; * * * (Silliman's Journal, vol. xx. p. 31,) the obstruction which they afford produces a constant tendency to circular evolution. * * * These masses of atmosphere thus set into active revolution continue to sweep along the islands with increased rapidity of gyration until they impinge upon the American coast." " We have assumed that the leading storms of the northern and western Atlantic and theAmerican coast originate in detached and gyrating portions of the northern margin of the trade winds, occasioned by the oblique obstruction which is opposed by the islands to the direct progress of this part of the trade, or to the falling of the northerly and eddy wind upon the trade, or to these causes combined."—(Silliman's Journal, vol. xx. p. 48.)

67. I trust it will be sufficiently evident, that although great and regularly moving masses of air, by encountering obstructions, may undergo a transient deflection, and that a portion accidentally caught in a strait with high cliffs on either side might, like the tide in the Bay of Fundy, acquire a local and temporary acceleration, yet that it would be utterly impossible for a durable whirlwind to be thus excited. Obviously for the endurance of a whirl, if not for its production, the continuous application of at least two forces would be requisite, of which one must be endowed with a centripetal efficacy in order to counteract the concomitant centrifugal momentum. It will be evident that although a local obstruction may cause an eddy or whirl in its vicinity, the rotary momentum thus created must soon be exhausted. But admitting that a blast by being deflected by an island could become a permanent whirlwind, obviously the resulting velocity could not be so great as that of the generating current. The moderately blowing trade wind could not, by contact with an inert body, acquire an increase of velocity adequate to form a furious hurricane capable, as represented, of travelling circuitously for more than two thousand miles.

68. The hurricane once created, agreeably to the imagination of Mr. Redfield, its subsequent progress is described in

the following language:—"This progress still continues while the stormy mass is revolving around its own moving axis; and we can readily comprehend the violent effects of its unresisted rotation, while this velocity becomes accelerated by nearly all the oblique forces and perhaps resistances of the circumjacent currents or masses of moving atmosphere. These storms cover, at the same moment of time, an extent of contiguous surface, the diameter of which may vary from one to five hundred miles, and in some cases have been much more extensive. They act with diminished violence towards the exterior, and with increased energy towards the interior of the space which they occupy." (Silliman's Journal, vol. xxv. p. 114.)

69. Thus it is assumed, that a mass of air from "one to five hundred miles in diameter" being made to whirl with the velocity of a most furious gale, is not only "unresisted" by the waves, forests, hills and mountains which it may encounter, but is actually "accelerated by nearly all the oblique forces and perhaps RESISTANCES" which it may meet. Yet it must be quite clear, that any reaction with currents not moving the same way, or moving with an inferior velocity, or obliquely, could only be productive of retardation.

70. The following inconsistencies will show how far Mr. Redfield's account of the phænomena of storms is to be deemed sufficiently accurate or consistent to overset the established principles of science.

71. "The rotation of a continued whirlwind involves not only changes in the position and condition of its constituent particles, but a constant accession of the exterior atmosphere to the body of the whirlwind, together with a discharge equally constant spirally at one extremity of its axis of rotation." (Franklin Journal, vol. xix. p. 122.) Ibid., p. 120: "Nor is it my intention to deny any movement or upward tendency at the centre of a whirlwind storm, for of such a movement, apart from theory, I have long since obtained good evidence." Ibid., p. 122: "In regard to the depression of the barometer which I have ascribed to the rotary action of whirlwind storms, Mr. Espy has himself shown, that the centrifugal action in a storm which gyrates horizontally must tend to withdraw or rarefy the air at the centre by causing a transfer or accumulation towards the exterior of the storm, thus causing a higher state of the barometer around the exterior border, than at the centre of the gale. This connexion and result is in strict accordance with the facts of the case as exhibited in all storms of this character, so far as my observations and information extend."

72. On opposite sides of the same leaf we find the preceding quotations. Agreeably to the first, there is a constant accession of air from the exterior atmosphere to the body of a whirlwind, attended by an upward force and compensated by a discharge at one extremity of its axis of rotation; agreeably to the last, the centrifugal action tends to withdraw the air of the centre by causing a transfer or accumulation towards the exterior border.

73. In tornadoes the author admits the undeniable existence of an ascending column at the axis (92.), and we are told that a whirlwind storm " operates in the same manner and exhibits the same general characteristics as a tornado*;" but this idea is evidently irreconcilable with that of a withdrawal of air from the centre, agreeably to one of the contradictory allegations above cited.

74. Nor are the following observations more consistent. " During the passage of these eddies or storms over the place of observation the barometer sinks while under their first or more advanced portions and rises as they pass over or recede." (Silliman's Journal, vol. xxv. p. 129.) " The barometer, whether in higher or lower latitudes, always sinks while under the first portion or moiety on every part of its track excepting perhaps its extreme northern margin." " The mercury in the barometer always rises again during the last portion of the gale and commonly attains the maximum of its elevation on the entire departure of the storm."

75. But if " a higher state of the barometer be created around the exterior border of a whirlwind than at the centre," and if of necessity the exterior border be first encountered, how does it happen that precisely about this space, agreeably to the statement last quoted, the barometrical column should sink? And if, agreeably to the statement quoted previously, the air be rarefied about the centre and accumulated towards the border, in passing from the one border to the other through the centre, would not the mercury in the barometer first rise, then sink, and afterwards rise again, instead of falling during its exposure to one moiety of the storm, and rising during exposure to the other?

76. It may be presumed, that respecting the state of the barometer and the movement of the air, within the sphere of his whirlwinds, Mr. Redfield's views are not in accordance with any settled notions. His theory leads to the idea of a centrifugal force, rarefying and removing the air from the centre, while his observation of the ascending current in tornadoes has tended to create an opposite impression.

* Silliman's Journal, vol. xxv. p. 117.

77. Considering the inconsistencies of Mr. Redfield's "reliable facts and observations," I hope I may be allowed to show what ought to ensue according to his own premises. Evidently in a whirlwind, constituted as are those to which we have reference, the centrifugal force will cause an accumulation of air towards the exterior until the otherwise uncounteracted pressure of the accumulation, tending to restore the level, is *in equilibrio* with the centrifugal force. Moreover, the reaction of the fluid lying in the same plane beyond the whirl, will cause the fluid to be higher, or if elastic, denser at an intermediate point than the general level. In the case of an elastic fluid like the air, condensation will be substituted for accumulation, and will amount to the same thing in effect. It would follow, that as the whirl should advance, the barometer would rise until the front limb of the zone of greatest condensation should arrive; subsequently it would fall till the central space should arrive, and then another rise and subsequent fall would ensue during the approach and departure of the rear limb of the zone of greatest condensation.

78. One fact is mentioned among the contradictory evidence above quoted, which seems to be supported by universal experience. The barometrical column does fall at the commencement of a storm, and of course this fact does not accord with the idea that storms are whirlwinds produced by mechanical forces remote from the axis and attended necessarily by a centrifugal action which would accumulate the air towards the exterior.

79. Respecting another characteristic, the "reliable facts and observations of our theorist" are no less irreconcilable than in the case last considered. I allude to the changes in the direction of the wind which ensue from the commencement to the end of a hurricane, and especially on the outer limbs on each side of the line of progression.

80. Thus, speaking of the progress of a storm from south-west to north-east along the coast of the United States, he alleges that "along the central portions of the track the first force of the wind is from a point near south-east, but after blowing for a certain period it changes suddenly, and usually, after a short intermission, to a point nearly or directly opposite to that from which it has previously been blowing; from which opposite quarter it blows with equal violence till the storm has passed over or abated." Again, "It is demonstrably evident, that at any point over which the centre of a whirlwind may pass, the wind must suddenly change to a direction almost exactly opposite to that which has been felt during the preceding part of its progress." (Silliman's Journal,

vol. xx. p. 22.) "It sometimes happens, when the central portion of an extensive storm passes over or near the point of observation, that the comparative calm, or lull which prevails about the apparent centre of rotation is preceded by a gradual rather than a sudden abatement of the wind." "Every experienced navigator will shrink with instinctive apprehension from the very idea of those moments of awful stillness which place him in the central vortex of the hurricane." (Franklin Journal, vol. xix. p. 116, and Silliman's Journal, vol. xx. p. 47.)

81. Amid the neutralization of evidence which inevitably results from the conflicting statements above quoted, I will endeavour to point out the results which ought to ensue if the inferences of the advocates of the whirlwind doctrine were correct.

82. When a rotary motion is communicated to a solid by a force applied to any part whatever, the tangential velocity at any point will be directly as its distance from the centre. In a fluid, when the force productive of rotation is applied at any point remote from the axis, the motion at the axis can be no quicker than in the case of a solid, but may be slower, since the parts do not of necessity move simultaneously. In the case of a fluid body kept in motion by a momentum resulting from forces previously applied, as in the instance of a Redfield whirlwind, any zone, which has been made to revolve by the direct application of force, will be retarded until it causes, in the adjoining zones, a due proportionable velocity. This will not be attained until the whole rotates like a solid. There is however this difference, that the external portions of the whirling zone being pressed by the centrifugal force against other portions of the same fluid, the one will conflict with the other, so as to cause the velocity to be communicated and to lessen outwards from the zone (in which the moving power is or has been applied) till it becomes insensible. This result must ensue the more speedily, since the momentum receives no reinforcement, while the mass which it actuates increases with the square of the distance from the axis.

83. It follows that at any station over which, or near which the centre of a whirlwind shall pass, there will be a breeze scarcely perceptible at first, but which will strengthen gradually into a gale of pre-eminent fury. Subsequently a declension must take place until the centre arrives; here again there would be no perceptible wind. The centre having moved away, the wind must increase again to a maximum of force and then decline to a breeze.

84. Mr. Redfield alleges, that the storm of August 17th, 1830, whirling to the left, travelled from south-west to north-east at the rate nearly of twenty-seven miles per hour; that its greatest diameter was from five hundred to six hundred miles; that of its severe part was from one hundred and fifty to two hundred and fifty miles. Thus it may be assumed, that in order for an observer to be exposed successively within the severe portion on the south-eastern and north-western limbs, the storm would have had to move at least one hundred miles, requiring nearly four hours. Hence if the storm in question were a whirlwind, instead of the change having been sudden, several hours would have been required for its gradual accomplishment.

85. To prove therefore that a sudden change ensued from one violent wind to another of the same character blowing in an opposite direction, is to demonstrate that the storm in which it took place was not an extensive whirlwind. Yet this characteristic is universally admitted to belong to hurricanes, and especially to those upon our territory in which a south-easter is followed by a north-wester. Hence the seaman's saying which Mr. Redfield sanctions in quoting, "a north-wester does not remain long in debt to a south-easter."

86. But if the storm above alluded to moved from south-west to north-east as Mr. Redfield's doctrine requires, and the velocity of the wind on the south-eastern and north-western limbs of the whirl were as great as described, that on the south-western side must have been more than a fourth more violent, having the general motion of the storm superadded to its appropriate gyrating velocity. Yet there is no evidence that any such superiority existed. On the contrary, the violence of the south-easter and north-wester seems to have been pre-eminently the object of attention.

87. Agreeably to Mr. Redfield, hurricanes have a diameter varying from one mile to five hundred miles, the diameter of the severe part of the storm of August 1830, being from one hundred and fifty to two hundred and fifty miles. Of course a portion of the eastern as well as the western limb of such a storm might be comprised between the Alleghany mountains and the Atlantic shore; and in no case would the inner portion of the south-eastern and more violent limb be beyond the cognizance of our merchants and insurers. It would be a matter of course that in every violent north-east gale, arising as represented from the progression of the north-western limb along our coast, fears would be entertained lest vessels, inward-bound, should be met by a much more violent south-wester. But experience shows, that every north-easter brings in a

crowd of vessels having only to complain of the violence, not of the direction of the wind.

88. It has been assumed, that a storm whirling to the left and travelling north-easterly, must, at stations passing nearly under the centre, first blow as a south-easter and afterwards gradually change to a north-wester. Meanwhile on the south-eastern or left limb it will blow only from the south-west, and on the north-western or right limb it will blow only from the north-west. Consistently, when the storm travels from south-east to north-west, as hurricanes are represented to travel in proceeding from the sphere of their origin in the West Indies to the coast of North America, it will at stations within a certain distance of a line described by the centre, blow from the north-east first. On the south-western limb it will blow first as a north-wester; on the north-eastern limb as a south-easter. Moreover, that on the last-mentioned limb the greatest violence will occur, since the general motion of the whirlwind will there cooperate with that of the whirl. Yet in the following paragraph Mr. Redfield informs us (Silliman's Journal, vol. xxv. p. 128), that "in the West Indies hurricanes begin to blow from a northern quarter of the horizon, and then changing to west and round to a southern quarter and then their fury is over."

89. This account of the direction of the wind in West India hurricanes agrees with that quoted by Espy from Edwards's History of Jamaica, vol. iii.: "All hurricanes begin from the north, veer back to west-north-west, west, and south-south-west, and when got to south-east, the foul weather breaks up."

90. It must be evident, as stated among my "objections," that when a whirl is first originated, whether it describe a helix, as would result from its progressive circular motion, or a circle, as represented by Mr. Redfield in his charts*, it must at thirty-two stations equidistant from each other and the centre of gyration, blow from as many points of the compass. However, when once under way, it being granted that the whirling is always from right to left, evidently at any station near the line described by the centre, it will begin to blow at right angles to that line or from the north-east. As the centre advances this wind would gradually subside, and, after the centre should have gone by, it would begin to blow from the south-west with increasing force till the severe part of the south-eastern limb should be passed. On this part of the track only one change would take place. But at two stations sufficiently remote from the central line, the wind in passing from north-

* Franklin Journal, vol. xix. p. 120.

east to south-east would undergo an intermediate deviation, but necessarily of an opposite nature, since for the same reason that at one there would be first more northing and then more westing, at the other there would be more easting and more southing, *pari passu.* But on the outward north-eastern and north-western limbs, or in other words, on the right and on the left external borders, there would be no change. On the one it would blow from the north-west only, on the other only from the south-east. On this last-mentioned limb the blast would be pre-eminent in violence, since in that direction the gyrative and progressive motion of the whirlwind would concur.

91. Nevertheless, agreeably to the observations which have lifted the whirlwind theory above the reach of my strictures, hurricanes in the West Indies begin (at every place) from a northern quarter, and changing first west, and afterwards to a southern quarter, terminate their fury. Thus, agreeably to the evidence of Mr. Redfield, the fury of the hurricane is the least where, according to his hypothesis, it should be the greatest.

92. Having cited and endeavoured to show the futility of the only explanation which can be found in Mr. Redfield's essays of the mode in which whirlwinds are induced, I will quote a passage from which it would seem that they are supposed capable of being self-induced. Whence it would follow, that without any extraneous aid, his "rotary movement, which is the sole cause of destructive winds and tempests," could spontaneously excite itself and the adjoining elements into a destructive commotion. From this statement, it appears that the author was not aware that in making it he gave a blow to his favourite idea of opposing and unequal forces, arising from gravitation and terrestrial motion, being the cause of stormy atmospheric gyration.

93. "We may observe, also, that whirlwinds and spouts appear to commence gradually and to acquire their full activity without the aid of any foreign causes; and it is well known they are most frequent in those calm regions where apparently there are no active currents to meet each other, and they are least frequent where currents are in full activity." (Silliman's Journal, vol. xxxiii. p. 61.)

94. Treating of whirlwinds excited by fire, the author thus expresses himself:—"The foregoing results can only be explained by a violent vortical action steadily maintained. * * * The ascending power of the vortical column or whirlwind is strongly exhibited. * * * But the spire of a columnar vortex exhibits a penetrating and ascending power which far exceeds, both in its intensity and the extent of its action, any other as-

cending movement that we witness. This effect appears to be owing to the spiral motion of the column which presses onward in the direction of its axis, till it reaches a limit of elevation yet unknown." (Silliman's Journal, vol. xxxvi. p. 56.) Would it not be as reasonable to expect the spiral of iron usually employed to open bottles, spontaneously to penetrate a cork without being actuated by the operator's hand, as that the aërial spiral, which agreeably to the description above given, constitutes a tornado, should, "without any foreign aid," "or any currents to meet each other," be endowed with the force which he has described? Admitting the storm-producing efficacy of a collision between trade winds and islands, admitting that gravitation, and rotary and orbitual force are to be substituted for all other agency, how are those causes to extend influence to his aërial isolated spiral, so as to beget the wonderful vortical force portrayed?

95. I do not deem it expedient to enter upon any discussion as to the competency of the evidence by which the gyration of storms has been considered as proved. By Mr. Espy that has been ably contested. I have given some reasons for doubting the accuracy or consistency of Mr. Redfield's representations, though I have no doubt they have always been made in perfect good faith. I have already alleged, that were gyration sufficiently proved, I should consider it as an effect of a conflux to supply an upward current at the axis. Yet the survey of the New Brunswick tornado, made on *terra firma* with the aid of a compass, by an observer so skilful and unbiassed as Professor Bache, ought to outweigh maritime observations, made in many cases under circumstances of difficulty and danger. In like manner great credit should be given to the observations collected by Professor Loomis respecting a remarkable inland storm of December 1836. This storm commenced blowing between south and east to the westward of the Mississippi, and travelled from west or north-west to east or south-east at a rate of between thirty and forty miles per hour. There appears to have been within the sphere of its violence an area, throughout which the barometric column stood at a minimum, and towards which the wind blew violently on the one side only from between east and south, and on the other only between north and west. This area extended from south-west to north-east more than two thousand miles. Its great length in proportion to its breadth seems irreconcilable with its having formed the axis of a whirlwind. The course of this storm, as above stated, was at right angles to that attributed by Redfield to storms of this kind. (Trans. of the American Phil. Society, vol. vii.)

96. Having said so much against the whirlwind theory of storms, it may be expected that I should, on this occasion, say something respecting the opinions which I entertain of their origin. To a certain extent this will be found in my communications published in Silliman's Journal, vol. xxxii. p. 153, vol. xl. p. 137, also in my Essay on the Gales of the United States. I still believe our north-eastern gales were correctly represented in the last-mentioned essay as arising from an exchange of position made between the air of the Gulf of Mexico and that of the territory of the United States which lies to the north-east of that great estuary; and that the heat given out during the conversion of aqueous vapour into rain, by imparting to the atmosphere as much caloric as could be yielded by twice its weight of red-hot sand, is a great instrument in the production of the phænomena; also, that the cold resulting from rarefaction is a cause of the condensation of that vapour, and of course of clouds. On this last idea, derived from Dalton, Mr. Espy has founded his ingenious theory of storms; alleging, erroneously, as I think, the buoyancy, resulting from the heat thus evolved, to be the grand cause of rain, also of tornadoes, hurricanes, and other electrical storms. In the essay above mentioned, I erred in ascribing too much to variations of density arising from changes of elevation, and twenty years' additional experience as an experimenter in electricity, has taught me to ascribe vastly more to this agent than I did formerly.

97. In November last, I verified a conjecture of my friend Dr. J. K. Mitchell, that moist, foggy or cloudy air is not a conductor of electricity, its influence, in paralyzing the efficacy of electrical apparatus, arising from moisture deposited on adjoining solid surfaces.

98. A red-hot iron cylinder, upon which evidently no moisture could be deposited, suspended from the excited conductor of an electrical machine, was found to yield sparks within a receiver replete with aqueous vapour, arising from a capsule of boiling water.

99. Hence it appears that bodies of air, whether cloudy or clear, may be oppositely electrified from each other or from the earth. This would explain the gyration on a horizontal axis which seems to be attendant on thunder gusts, and may account for the ascent of the south-easter and descent of the north-wester in the great storm of December 1836, described by Professor Loomis.

100. Such gyration may be a form of convective discharge, in which electrical reaction is assisted by calorific circulation and the evolution of latent heat, agreeably to Dalton and Espy.

101. Squalls may be the consequence of electrical reaction between the terrestrial surface and oppositely excited masses of air, and the intermixture of masses so excited in obedience to the same cause, may be among the sources of rain, hail, and gusts. The specific gravity of a body of air, electrified differently from the surrounding medium, may be lessened by what is called electric repulsion; the particles inevitably moving a greater distance from each other, as similarly electrified pith-balls are known to do.

102. Hence a cause of rarefaction, buoyancy, and consequent upward motion, in a column of electrified air, more competent than that suggested by Espy.

103. Should it be verified that a gyration from right to left takes place during convective discharges of electricity in hurricanes, it may be referrible to the disposition which a positive electrical discharge from the earth to the sky would have to gyrate in that direction.

I have prepared some strictures on Dove's Essay on the Law of Storms, which will be the subject of a future communication.

XVI. *On a Method of Etching on Hardened Steel Plates and other Polished Metallic Surfaces by means of Electricity. By* J. H. Pring, *M.D.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

I HEREWITH transmit to you a rough specimen of what I conceive to be a novel employment of the power of electricity, and shall be gratified should the process by which it was effected prove susceptible of any useful application to the arts.

The method which I employed in the production of the characters on the accompanying plate * was the following:—Having six batteries of the kind invented by Mr. Smee, the platinized silver plate of each being about three inches square, I attached the steel plate to be etched upon to the zinc extremity of the batteries, a coil of covered wire, of considerable length, being previously interposed between the steel plate and the zinc: then taking the wire connected with the platinized silver in my hand, I used it as an etching-tool on the steel plate,—an electrical spark of great brilliancy, accom-

* This was a steel plate, on which the words "Etched by means of Electricity. Bath, 30th June 1843. I. H. P." together with some ornamental devices, had been produced by the above method. It gave only a faint, just legible impression by the copper-plate press.—Edit.

panied by a slight indentation on the steel, was the result of each contact of the wire with the plate.

The wire by which the etching was made was of platina; the part at which it was held was carried through a glass tube for the purposes of affording a more convenient handle, and of protecting the hand from shocks to which it might otherwise have been exposed.

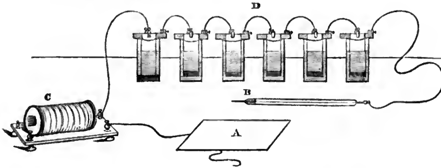
By using the wire connected with the zinc of the batteries as the etching-tool, and attaching the steel plate to the platinized silver, a very different effect is produced. With the apparatus thus arranged, the spark that results from the contact of the wire with the steel plate is accompanied by a deposition of a minute portion of the substance of the wire on the steel; by using different wires, therefore, as of gold, silver, platina, &c., a variety of ornamental designs may probably be formed on polished steel surfaces.

The effect of the electrical agency here described is not however confined to steel; a somewhat similar one may be obtained by substituting plates of other metals. By augmenting the quantity and intensity of the electrical current, it seems probable that the effect on the steel, or other metals, would be proportionally increased; and it may be anticipated that, by other modifications of the process, its applications may be advantageously extended.

I remain, Sir, yours respectfully,

Bath, July 5, 1843. JAMES HURLY PRING, M.D.

The accompanying sketch, in which the apparatus is represented lying ready for use, may perhaps serve to illustrate the foregoing description.



A. The steel, or other metallic plate to be etched upon.
B. The etching point of platina wire projecting from the glass handle.
C. The coil of covered wire. D. The batteries.

XVII.—*Series of Propositions for rendering the Nomenclature of Zoology uniform and permanent, being the Report of a Committee for the consideration of the subject appointed by the British Association for the Advancement of Science**.

ALL persons who are conversant with the present state of Zoology must be aware of the great detriment which the science sustains from the vagueness and uncertainty of its nomenclature. We do not here refer to those diversities of language which arise from the various methods of classification adopted by different authors, and which are unavoidable in the present state of our knowledge. So long as naturalists differ in the views which they are disposed to take of the natural affinities of animals there will always be diversities of classification, and the only way to arrive at the true system of nature is to allow perfect liberty to systematists in this respect. But the evil complained of is of a different character. It consists in this, that when naturalists *are* agreed as to the characters and limits of an individual group or species, they still disagree in the appellations by which they distinguish it. A genus is often designated by three or four, and a species by twice that number of precisely equivalent synonyms; and in the absence of any rule on the subject, the naturalist is wholly at a loss what nomenclature to adopt. The consequence is, that the so-called commonwealth of science is becoming daily divided into independent states, kept asunder by diversities of language as well as by geographical limits. If an English zoologist, for example, visits the museums and converses with the professors of France, he finds that their *scientific* language is almost as foreign to him as their *vernacular*. Almost every specimen which he examines is labeled by a title which is unknown to him, and he feels that nothing short of a continued residence in that country can make him conversant with her science. If he proceeds thence to Germany or Russia, he is again at a loss: bewildered everywhere amidst the confusion of nomenclature, he returns in despair to his own country and to the museums and books to which he is accustomed.

If these diversities of scientific language were as deeply rooted as the vernacular tongue of each country, it would of course be hopeless to think of remedying them; but happily this is not the case. The language of science is in the mouths of comparatively few, and these few, though scattered over distant lands, are in habits of frequent and friendly intercourse with each other. All that is wanted then is, that some plain and simple regulations, founded on justice and sound reason, should be drawn up by a competent body of persons, and then be extensively distributed throughout the zoological world.

The undivided attention of chemists, of astronomers, of anatomists, of mineralogists, has been of late years devoted to fixing their respective lan-

* From the Report of the Association for 1842, p. 105. The Committee appointed by the Council, Feb. 11, 1842, consisted of the following members:—Mr. Darwin, Prof. Henslow, Rev. L. Jenyns, Mr. Ogilby, Mr. J. Phillips, Dr. Richardson, Mr. H. E. Strickland (reporter), and Mr. Westwood: to whom were subsequently added Messrs. Broderip, Prof. Owen, Shuckard, Waterhouse and Yarrell. The Report states that an outline of the proposed rules having been drawn up, copies were sent to eminent zoologists at home and abroad, with a request that they would favour the Committee with their comments; and that many valuable suggestions had already been thus obtained.—Ed.

guages on a sound basis. Why, then, do zoologists hesitate in performing the same duty? at a time, too, when all acknowledge the evils of the present anarchical state of their science.

It is needless to inquire far into the causes of the present confusion of zoological nomenclature. It is in great measure the result of the same branch of science having been followed in distant countries by persons who were either unavoidably ignorant of each other's labours, or who neglected to inform themselves sufficiently of the state of the science in other regions. And when we remark the great obstacles which now exist to the circulation of books beyond the conventional limits of the states in which they happen to be published, it must be admitted that this ignorance of the writings of others, however unfortunate, is yet in great measure pardonable. But there is another source for this evil, which is far less excusable,—the practice of gratifying individual vanity by attempting on the most frivolous pretexts to cancel the terms established by original discoverers, and to substitute a new and unauthorized nomenclature in their place. One author lays down as a rule, that no specific names should be derived from geographieal sources, and unhesitatingly proceeds to insert words of his own in all such cases; another declares war against names of exotic origin, foreign to the Greek and Latin; a third excommunicates all words which exceed a certain number of syllables; a fourth cancels all names which are complimentary of individuals, and so on, till universality and permanence, the two great essentials of scientifie language, are utterly destroyed.

It is surely, then, an object well worthy the attention of the Zoological Section of the British Association for the Advancement of Science, to devise some means which may lessen the extent of this evil, if not wholly put an end to it. The best method of making the attempt seems to be, to entrust to a carefully selected committee the preparation of a series of rules, the adoption of which must be left to the sound sense of naturalists in general. By emanating from the British Association, it is hoped that the proposed rules will be invested with an authority which no individual zoologist, however eminent, could confer on them. The world of science is no longer a monarchy, obedient to the ordinances, however just, of an Aristotle or a Linnæus. She has now assumed the form of a republic, and although this revolution may have increased the vigour and zeal of her followers, yet it has destroyed much of her former order and regularity of government. The latter can only be restored by framing such laws as shall be based in reason and sanctioned by the approval of men of science; and it is to the preparation of these laws that the Zoological Section of the Association have been invited to give their aid.

In venturing to propose these rules for the guidance of all classes of zoologists in all countries, we disclaim any intention of dictating to men of science the course which they may see fit to pursue. It must of course be always at the option of authors to adhere to or depart from these principles, but we offer them to the candid consideration of zoologists, in the hope that they may lead to sufficient uniformity of method in future to rescue the science from becoming a mere chaos of words.

We now proceed to develope the details of our plan; and in order to make the reasons by which we are guided apparent to naturalists at large, it will be requisite to append to each proposition a short explanation of the circumstances which call for it.

Among the numerous rules for nomenclature which have been proposed by naturalists, there are many which, though excellent in themselves, it is not now desirable to enforce*. The cases in which those rules have been overlooked or departed from, are so numerous and of such long standing, that to carry these regulations into effect would undermine the edifice of zoological nomenclature. But while we do not adopt these propositions as authoritative laws, they may still be consulted with advantage in making such additions to the language of zoology as are required by the progress of the science. By adhering to sound principles of philology, we may avoid errors in future, even when it is too late to remedy the past, and the language of science will thus eventually assume an aspect of more classic purity than it now presents.

Our subject hence divides itself into two parts; the first consisting of ***Rules*** for the rectification of the present zoological nomenclature, and the second of ***Recommendations*** for the improvement of zoological nomenclature in future.

PART I.

RULES FOR RECTIFYING THE PRESENT NOMENCLATURE.

[*Limitation of the Plan to Systematic Nomenclature.*]

In proposing a measure for the establishment of a permanent and universal zoological nomenclature, it must be premised that we refer solely to the Latin or systematic language of zoology. We have nothing to do with vernacular appellations. One great cause of the neglect and corruption which prevails in the scientific nomenclature of zoology, has been the frequent and often exclusive use of vernacular names in lieu of the Latin binomial designations, which form the only legitimate language of systematic zoology. Let us then endeavour to render perfect the Latin or Linnæan method of nomenclature, which, being far removed from the scope of national vanities and modern antipathies, holds out the only hope of introducing into zoology that grand desideratum, an universal language.

[*Law of Priority the only effectual and just one.*]

It being admitted on all hands that words are only the conventional signs of ideas, it is evident that language can only attain its end effectually by being permanently established and generally recognized. This consideration ought, it would seem, to have checked those who are continually attempting to subvert the established language of zoology by substituting terms of their own coinage. But, forgetting the true nature of language, they persist in confounding the *name* of a species or group with its *definition*; and because the former often falls short of the fullness of expression found in the latter, they cancel it without hesitation, and introduce some new term which appears to them more characteristic, but which is utterly unknown to the science, and is therefore devoid of all authority†. If these persons were to object to such names of men as *Long*, *Little*, *Armstrong*, *Golightly*, &c., in cases where they fail to apply to the individuals who bear them, or should complain of the names *Gough*, *Lawrence*, or *Harvey*, that they were devoid of meaning, and should hence propose to change them for more characteristic appella-

* See especially the admirable code proposed in the 'Philosophia Botanica' of Linnæus. If zoologists had paid more attention to the principles of that code, the present attempt at reform would perhaps have been unnecessary.

† Linnæus says on this subject, "Abstinendum ab hac innovatione quæ nunquam cessaret, quin indies aptiora detegerentur ad infinitum."

tions, they would not act more unphilosophically or inconsiderately than they do in the case before us; for, in truth, it matters not in the least by what conventional sound we agree to designate an individual object, provided the sign to be employed be stamped with such an authority as will suffice to make it pass current. Now in zoology no one person can subsequently claim an authority equal to that possessed by the person who is the first to define a new genus or describe a new species; and hence it is that the name originally given, even though it may be inferior in point of elegance or expressiveness to those subsequently proposed, ought as a general principle to be permanently retained. To this consideration we ought to add the injustice of erasing the name originally selected by the person to whose labours we owe our first knowledge of the object; and we should reflect how much the permission of such a practice opens a door to obscure pretenders for dragging themselves into notice at the expense of original observers. Neither can an author be permitted to alter a name which he himself has once published, except in accordance with fixed and equitable laws. It is well observed by Decandolle, " L'auteur même qui a le premier établi un nom n'a pas plus qu'un autre le droit de le changer pour simple cause d'impropriété. La priorité en effet est un terme fixe, positif, qui n'admet rien, ni d'arbitraire, ni de partial."

For these reasons, we have no hesitation in adopting as our fundamental maxim, the " law of priority," viz.

§ 1. The name originally given by the founder of a group or the describer of a species should be permanently retained, to the exclusion of all subsequent synonyms (with the exceptions about to be noticed).

Having laid down this principle, we must next inquire into the limitations which are found necessary in carrying it into practice.

[*Not to extend to authors older than Linnæus.*]

As our subject matter is strictly confined to the *binomial system of nomenclature*, or that which indicates species by means of two Latin words, the one generic, the other specific, and as this invaluable method originated solely with Linnæus, it is clear that, as far as species are concerned, we ought not to attempt to carry back the principle of priority beyond the date of the 12th edition of the 'Systema Naturæ.' Previous to that period, naturalists were wont to indicate species not by a *name* comprised in one word, but by a *definition* which occupied a sentence, the extreme verbosity of which method was productive of great inconvenience. It is true that one word sometimes sufficed for the definition of a species, but these rare cases were only binomial by accident and not by principle, and ought not therefore in any instance to supersede the binomial designations imposed by Linnæus.

The same reasons apply also to generic names. Linnæus was the first to attach a definite value to genera, and to give them a systematic character by means of exact definitions; and therefore although the *names* used by previous authors may often be applied with propriety to modern genera, yet in such cases they acquire a new meaning, and should be quoted on the authority of the first person who used them in this secondary sense. It is true, that several of the old authors made occasional approaches to the Linnæan exactness of generic definition, but still these were but partial attempts; and it is certain that if in our rectification of the binomial nomenclature we once

trace back our authorities into the obscurity which preceded the epoch of its foundation, we shall find no resting-place or fixed boundary for our researches. The nomenclature of Ray is chiefly derived from that of Gesner and Aldrovandus, and from these authors we might proceed backward to Ælian, Pliny, and Aristotle, till our zoological studies would be frittered away amid the refinements of classical learning*.

We therefore recommend the adoption of the following proposition:—

§ 2. The binomial nomenclature having originated with Linnæus, the law of priority, in respect of that nomenclature, is not to extend to the writings of antecedent authors.

[It should be here explained, that Brisson, who was a contemporary of Linnæus and acquainted with the 'Systema Naturæ,' defined and published certain genera of birds which are *additional* to those in the 12th edition of Linnæus's work, and which are therefore of perfectly good authority. But Brisson still adhered to the old mode of designating species by a sentence instead of a word, and therefore while we retain his defined genera, we do not extend the same indulgence to the titles of his species, even when the latter are accidentally binomial in form. For instance, the *Perdix rubra* of Brisson is the *Tetrao rufus* of Linnæus; therefore as we in this case retain the generic name of Brisson and the specific name of Linnæus, the correct title of the species would be *Perdix rufa*.]

[*Generic names not to be cancelled in subsequent subdivisions.*]

As the number of known species which form the groundwork of zoological science is always increasing, and our knowledge of their structure becomes more complete, fresh generalizations continually occur to the naturalist, and the number of genera and other groups requiring appellations is ever becoming more extensive. It thus becomes necessary to subdivide the contents of old groups and to make their definitions continually more restricted. In carrying out this process, it is an act of justice to the original author, that his generic name should never be lost sight of; and it is no less essential to the welfare of the science, that all which is sound in its nomenclature should remain unaltered amid the additions which are continually being made to it. On this ground we recommend the adoption of the following rule:—

§ 3. A generic name when once established should never be cancelled in any subsequent subdivision of the group, but retained in a restricted sense for one of the constituent portions.

[*Generic names to be retained for the typical portion of the old genus.*]

When a genus is subdivided into other genera, the original name should be retained for that portion of it which exhibits in the greatest degree its essential characters as at first defined. Authors frequently indicate this by selecting some one species as a fixed point of reference, which they term the "type of the genus." When they omit doing so, it may still in many cases be correctly inferred that the *first* species mentioned on their list, if found accurately to agree with their definition, was regarded by them as the type. A specific name or its synonyms will also often serve to point out the particular species which by implication must be regarded as the original type of a genus. In such cases we are justified in restoring the name of the old genus

* "Quis longo ævo recepta vocabula commutaret hodie cum patrum?"—*Linnæus.*

to its typical signification, even when later authors have done otherwise. We submit therefore that

§ 4. The generic name should always be retained for that portion of the original genus which was considered typical by the author.

Example.—The genus *Picumnus* was established by Temminck, and included two groups, one with four toes, the other with three, the *former* of which was regarded by the author as typical. Swainson, however, in raising these groups at a later period to the rank of genera, gave a new name, *Asthenurus*, to the *former* group, and retained *Picumnus* for the *latter*. In this case we have no choice but to restore the name *Picumnus*, Tem., to its correct sense, cancelling the name *Asthenurus*, Sw., and imposing a new name on the 3-toed group which Swainson had called *Picumnus*.

[*When no type is indicated, then the original name is to be kept for that subsequent subdivision which* first *received it.*]

Our next proposition seems to require no explanation:—

§ 5. When the evidence as to the original type of a genus is not perfectly clear and indisputable, then the person who first subdivides the genus may affix the original name to any portion of it at his discretion, and no later author has a right to transfer that name to any other part of the original genus.

[*A later name of the same extent as an earlier to be wholly cancelled.*]

When an author infringes the law of priority by giving a new name to a genus which has been properly defined and named already, the only penalty which can be attached to this act of negligence or injustice, is to expel the name so introduced from the pale of the science. It is not right then in such cases to restrict the meaning of the later name so that it may stand side by side with the earlier one, as has sometimes been done. For instance, the genus *Monaulus*, Vieill. 1816, is a precise equivalent to *Lophophorus*, Tem. 1813, both authors having adopted the same species as their type, and therefore when the latter genus came in the course of time to be divided into two, it was incorrect to give the condemned name *Monaulus* to one of the portions. To state this succinctly,

§ 6. When two authors define and name the same genus, *both making it exactly of the same extent*, the later name should be cancelled *in toto*, and not retained in a modified sense*.

This rule admits of the following exception:—

§ 7. Provided however, that if these authors select their respective types from different sections of the genus, and these sections be afterwards raised into genera, then both these names may be retained in a restricted sense for the new genera respectively.

Example.—The names *Œdemia* and *Melanetta* were originally co-extensive synonyms, but their respective types were taken from different sections which are now raised into genera, distinguished by the above titles.

[No special rule is required for the cases in which the later of two generic

* These discarded names may however be *tolerated*, if they have been afterwards proposed in a totally new sense, though we trust that in future no one will *knowingly* apply an old name, whether now adopted or not, to a new genus. (See proposition *q*, *infra*.)

names is so defined as to be *less extensive* in signification than the earlier, for if the later includes the type of the earlier genus, it would be cancelled by the operation of § 4; and if it does not include that type, it is in fact a distinct genus.]

But when the later name is *more extensive* than the earlier, the following rule comes into operation:—

[*A later name equivalent to several earlier ones is to be cancelled.*]

The same principle which is involved in § 6, will apply to § 8.

§ 8. If the later name be so defined as to be equal in extent to two or more previously published genera, it must be cancelled *in toto*.

Example.—Psarocolius, Wagl. 1827, is equivalent to five or six genera previously published under other names, therefore *Psarocolius* should be cancelled.

If these previously published genera be *separately adopted* (as is the case with the equivalents of *Psarocolius*), their original names will of course prevail; but if we follow the later author in combining them into one, the following rule is necessary:—

[*A genus compounded of two or more previously proposed genera whose characters are now deemed insufficient, should retain the name of one of them.*]

It sometimes happens that the progress of science requires two or more genera, founded on insufficient or erroneous characters, to be combined together into one. In such cases the law of priority forbids us to cancel *all* the original names and impose a *new* one on this compound genus. We must therefore select some one species as a type or example, and give the generic name which it formerly bore to the whole group now formed. If these original generic names differ in date, the oldest one should be the one adopted.

§ 9. In compounding a genus out of several smaller ones, the earliest of them, if otherwise unobjectionable, should be selected, and its former generic name be extended over the new genus so compounded.

Example.—The genera *Accentor* and *Prunella* of Vieillot not being considered sufficiently distinct in character, are now united under the general name of *Accentor*, that being the earliest. So also *Cerithium* and *Potamides*, which were long considered distinct, are now united, and the latter name merges into the former.

We now proceed to point out those few cases which form exceptions to the law of priority, and in which it becomes both justifiable and necessary to alter the names originally imposed by authors.

[*A name should be changed when previously applied to another group which still retains it.*]

It being essential to the binomial method to indicate objects in natural history by means of *two words* only, without the aid of any further designation, it follows that a generic name should only have one meaning, in other words, that two genera should never bear the same name. For a similar reason, no two species in the same genus should bear the same name. When these cases occur, the later of the two duplicate names should be cancelled, and a new term, or the earliest synonym, if there be any, substituted. When iti s necessary to form new words for this purpose, it is desirable to make them bear some analogy to those which they are destined to supersede, as where the genus of birds, *Plectorhynchus*, being preoccupied in Ichthyology

is changed to *Plectorhamphus*. It is, we conceive, the bounden duty of an author when naming a new genus, to ascertain by careful search that the name which he proposes to employ has not been previously adopted in other departments of natural history*. By neglecting this precaution he is liable to have the name altered and his authority superseded by the first subsequent author who may detect the oversight, and for this result, however unfortunate, we fear there is no remedy, though such cases would be less frequent if the detectors of these errors would, as an act of courtesy, point them out to the author himself, if living, and leave it to him to correct his own inadvertencies. This occasional hardship appears to us to be a less evil than to permit the practice of giving the same generic name *ad libitum* to a multiplicity of genera. We submit therefore, that

§ 10. A name should be changed which has before been proposed for some other genus in zoology or botany, or for some other species in the same genus, when still retained for such genus or species.

[*A name whose meaning is glaringly false may be changed.*]

Our next proposition has no other claim for adoption than that of being a concession to human infirmity. If such proper names of places as Covent Garden, Lincoln's Inn Fields, Newcastle, Bridgewater, &c., no longer suggest the ideas of gardens, fields, castles, or bridges, but refer the mind with the quickness of thought to the particular localities which they respectively designate, there seems no reason why the proper names used in natural history should not equally perform the office of correct indication even when their etymological meaning may be wholly inapplicable to the object which they typify. But we must remember that the language of science has but a limited currency, and hence the words which compose it do not circulate with the same freedom and rapidity as those which belong to every-day life. The attention is consequently liable in scientific studies to be diverted from the contemplation of the thing signified to the etymological meaning of the sign, and hence it is necessary to provide that the latter shall not be such as to propagate actual error. Instances of this kind are indeed very rare, and in some cases, such as that of *Monodon, Caprimulgus, Paradisea apoda* and *Monoculus*, they have acquired sufficient currency no longer to cause error, and are therefore retained without change. But when we find a Batrachian reptile named in violation of its true affinities, *Mastodonsaurus*, a Mexican species termed (through erroneous information of its habitat) *Picus cafer*, or an olive-coloured one *Muscicapa atra*, or when a name is derived from an accidental monstrosity, as in *Picus semirostris* of Linnæus, and *Helix disjuncta* of Turton, we feel justified in cancelling these names, and adopting that synonym which stands next in point of date. At the same time we think it right to remark that this privilege is very liable to abuse, and ought therefore to be applied only to extreme cases and with great caution. With these limitations we may concede that

§ 11. A name may be changed when it implies a false proposition which is likely to propagate important errors.

[*Names not clearly defined may be changed.*]

Unless a species or group is intelligibly defined when the name is given, it cannot be recognized by others, and the signification of the name is conse-

* This laborious and difficult research will in future be greatly facilitated by the very useful work of M. Agassiz, entitled "Nomenclator Zoologicus."

quently lost. Two things are necessary before a zoological term can acquire any authority, viz. *definition* and *publication*. Definition properly implies a distinct exposition of essential characters, and in all cases we conceive this to be indispensable, although some authors maintain that a mere enumeration of the component species, or even of a single type, is sufficient to authenticate a genus. To constitute *publication*, nothing short of the insertion of the above particulars *in a printed book* can be held sufficient. Many birds, for instance, in the Paris and other continental museums, shells in the British Museum (in Dr. Leach's time), and fossils in the Scarborough and other public collections, have received MS. names which will be of no authority until they are published*. Nor can any unpublished descriptions, however exact (such as those of Forster, which are still shut up in a MS. at Berlin), claim any right of priority till published, and then only from the date of their publication. The same rule applies to cases where groups or species are published, but not defined, as in some museum catalogues, and in Lesson's 'Traité d'Ornithologie,' where many species are enumerated by name, without any description or reference by which they can be identified. Therefore

§ 12. A name which has never been clearly defined in some published work should be changed for the earliest name by which the object shall have been so defined.

[*Specific names, when adopted as generic, must be changed.*]

The necessity for the following rule will be best illustrated by an example. The *Corvus pyrrhocorax*, Linn., was afterwards advanced to a genus under the name of *Pyrrhocorax*. Temminck adopts this generic name, and also retains the old specific one, so that he terms the species *Pyrrhocorax pyrrhocorax*. The inelegance of this method is so great as to demand a change of the specific name, and the species now stands as *Pyrrhocorax alpinus*, Vieill. We propose therefore that

§ 13. A new specific name must be given to a species when its old name has been adopted for a genus which includes that species.

N.B. It will be seen, however, below, that we strongly object to the further continuance of this practice of elevating specific names into generic.

[*Latin orthography to be adhered to.*]

On the subject of orthography it is necessary to lay down one proposition,—

§ 14. In writing zoological names the rules of Latin orthography must be adhered to.

In Latinizing Greek words there are certain rules of orthography known to classical scholars which must never be departed from. For instance, the names which modern authors have written *Aipunemia*, *Zenophasia*, *poiocephala*, must, according to the laws of etymology, be spelt *Æpycnemia*, *Xenophasia* and *pœocephala*. In Latinizing modern words the rules of classic usage do not apply, and all that we can do is to give to such terms as classical an appearance as we can, consistently with the preservation of their etymology. In the case of European words whose orthography is fixed, it is best to retain the original form, even though it may include letters and combinations unknown in Latin. Such words, for instance, as *Woodwardi*,

* These MS. names are in all cases liable to create confusion, and it is therefore much to be desired that the practice of using them should be avoided in future.

Knighti, Bullocki, Eschscholtzi, would be quite unintelligible if they were Latinized into *Vudvardi, Cnichti, Bullocci, Essolzi,* &c. But words of barbarous origin, having no fixed orthography, are more pliable, and hence, when adopted into the Latin, they should be rendered as classical in appearance as is consistent with the preservation of their original sound. Thus the words *Tockus, awsuree, argoondah, kundoo,* &c. should, when Latinized, have been written *Toccus, ausure, argunda, cundu,* &c. Such words ought, in all practicable cases, to have a Latin termination given them, especially if they are used generically.

In Latinizing proper names, the simplest rule appears to be to use the termination *-us,* genitive *-i,* when the name ends with a consonant, as in the above examples; and *-ius,* gen. *-ii,* when it ends with a vowel, as *Latreille, Latreillii,* &c.

In converting Greek words into Latin the following rules must be attended to:—

Greek.		Latin.	Greek.		Latin.
αι	becomes	æ.	θ	becomes	th.
ει	„	i.	φ	„	ph.
ος	terminal,	us.	χ	„	ch.
ον	„	um.	κ	„	c.
ου	becomes	u.	γχ	„	nch.
οι	„	œ.	γγ	„	ng.
υ	„	y.	‛	„	h.

When a name has been erroneously written and its orthography has been afterwards amended, we conceive that the authority of the original author should still be retained for the name, and not that of the person who makes the correction.

PART II.

RECOMMENDATIONS FOR IMPROVING THE NOMENCLATURE IN FUTURE.

The above propositions are all which in the present state of the science it appears practicable to invest with the character of laws. We have endeavoured to make them as few and simple as possible, in the hope that they may be the more easily comprehended and adopted by naturalists in general. We are aware that a large number of other regulations, some of which are hereafter enumerated, have been proposed and acted upon by various authors who have undertaken the difficult task of legislating on this subject; but as the enforcement of such rules would in many cases undermine the invaluable principle of priority, we do not feel justified in adopting them. At the same time we fully admit that the rules in question are, for the most part, founded on just criticism, and therefore, though we do not allow them to operate retrospectively, we are willing to retain them for future guidance. Although it is of the first importance that the principle of priority should be held paramount to all others, yet we are not blind to the desirableness of rendering our scientific language palatable to the scholar and the man of taste. Many zoological terms, which are now marked with the stamp of perpetual currency, are yet so far defective in construction, that our inability to remove them without infringing the law of priority may be a subject of regret. With these terms we cannot interfere, if we adhere to the principles above laid down; nor is there even any remedy, if authors insist on infringing the rules of good taste by introducing into the science words of the same inelegant or unclassical character in future. But that which cannot be enforced by law may, in some

measure, be effected by persuasion; and with this view we submit the following propositions to naturalists, under the title of *Recommendations for the improvement of Zoological Nomenclature in future.*

[*The best names are Latin or Greek characteristic words.*]

The classical languages being selected for zoology, and words being more easily remembered in proportion as they are expressive, it is self-evident that

§ A. The *best* zoological names are those which are derived from the Latin or Greek, and express some distinguishing characteristic of the object to which they are applied.

[*Classes of objectionable names.*]

It follows from hence that the following classes of words are more or less objectionable in point of taste, though, in the case of *genera*, it is often necessary to use them, from the impossibility of finding characteristic words which have not before been employed for other genera. We will commence with those which appear the least open to objection, such as

a. Geographical names.—These words being for the most part adjectives can rarely be used for *genera*. As designations of *species* they have been so strongly objected to, that some authors (Wagler, for instance) have gone the length of substituting fresh names wherever they occur; others (*e.g.* Swainson) will only tolerate them where they apply *exclusively*, as *Lepus hibernicus*, *Troglodytes europæus*, &c. We are by no means disposed to go to this length. It is not the less true that the *Hirundo javanica* is a Javanese bird, even though it may occur in other countries also, and though other species of *Hirundo* may occur in Java. The utmost that can be urged against such words is, that they do not tell the *whole truth*. However, as so many authors object to this class of names, it is better to avoid giving them, except where there is reason to believe that the species is chiefly confined to the country whose name it bears.

b. Barbarous names.—Some authors protest strongly against the introduction of exotic words into our Latin nomenclature, others defend the practice with equal warmth. We may remark, first, that the practice is not contrary to classical usage, for the Greeks and Romans did occasionally, though with reluctance, introduce barbarous words in a modified form into their respective languages. Secondly, the preservation of the trivial names which animals bear in their native countries is often of great use to the traveller in aiding him to discover and identify species. We do not therefore consider, if such words have a Latin termination given to them, that the occasional and judicious use of them as scientific terms can be justly objected to.

c. Technical names.—All words expressive of trades and professions have been by some writers excluded from zoology, but without sufficient reason. Words of this class, *when carefully chosen*, often express the peculiar characters and habits of animals in a metaphorical manner, which is highly elegant. We may cite the generic terms *Arvicola*, *Lanius*, *Pastor*, *Tyrannus*, *Regulus*, *Mimus*, *Ploceus*, &c., as favourable examples of this class of names.

d. Mythological or historical names.—When these have no perceptible reference or allusion to the characters of the object on which they are conferred, they may be properly regarded as unmeaning and in bad taste. Thus the generic names *Lesbia*, *Leilus*, *Remus*, *Corydon*, *Pasiphae*, have been applied to a Humming bird, a Butterfly, a Beetle, a Parrot, and a Crab respectively,

without any perceptible association of ideas. But mythological names may sometimes be used as generic with the same propriety as technical ones, in cases where a direct allusion can be traced between the narrated actions of a personage and the observed habits or structure of an animal. Thus when the name *Progne* is given to a Swallow, *Clotho* to a Spider, *Hydra* to a Polyp, *Athene* to an Owl, *Nestor* to a grey-headed Parrot, &c., a pleasing and beneficial connexion is established between classical literature and physical science.

e. Comparative names.—The objections which have been raised to words of this class are not without foundation. The names, no less than the definitions of objects, should, where practicable, be drawn from positive and self-evident characters, and not from a comparison with other objects, which may be less known to the reader than the one before him. Specific names expressive of comparative size are also to be avoided, as they may be rendered inaccurate by the after-discovery of additional species. The names *Picoides*, *Emberizoides*, *Pseudoluscinia*, *rubeculoides*, *maximus*, *minor*, *minimus*, &c. are examples of this objectionable practice.

f. Generic names compounded from other genera.—These are in some degree open to the same imputation as comparative words; but as they often serve to express the position of a genus as intermediate to, or allied with, two other genera, they may occasionally be used with advantage. Care must be taken not to adopt such compound words as are of too great length, and not to corrupt them in trying to render them shorter. The names *Gallopavo*, *Tetraogallus*, *Gypaetos*, are examples of the appropriate use of compound words.

g. Specific names derived from persons.—So long as these complimentary designations are used with moderation, and are restricted to persons of eminence as scientific zoologists, they may be employed with propriety in cases where expressive or characteristic words are not to be found. But we fully concur with those who censure the practice of naming species after persons of no scientific reputation, as curiosity dealers (e. g. *Caniveti*, *Boissoneauti*), Peruvian priestesses (*Cora*, *Amazilia*), or Hottentots (*Klassi*).

h. Generic names derived from persons.—Words of this class have been very extensively used in botany, and therefore it would have been well to have excluded them wholly from zoology, for the sake of obtaining a *memoria technica* by which the name of a genus would at once tell us to which of the kingdoms of nature it belonged. Some few personal generic names have however crept into zoology, as *Cuvieria*, *Mulleria*, *Rossia*, *Lessonia*, &c., but they are very rare in comparison with those of botany, and it is perhaps desirable not to add to their number.

i. Names of harsh and inelegant pronunciation.—These words are grating to the ear, either from inelegance of form, as *Huhua*, *Yuhina*, *Craxirex*, *Eschscholtzi*, or from too great length, as *chirostrongylostinus*, *Opetiorhynchus*, *brachypodioides*, *Thecodontosaurus*, not to mention the *Enaliolimnosaurus crocodilocephaloides* of a German naturalist. It is needless to enlarge on the advantage of consulting euphony in the construction of our language. As a general rule it may be recommended to avoid introducing words of more than five syllables.

k. Ancient names of animals applied in a wrong sense.—It has been customary, in numerous cases, to apply the names of animals found in classic authors at random to exotic genera or species which were wholly unknown to the ancients. The names *Cebus*, *Callithrix*, *Spiza*, *Kitta*, *Struthus*, are examples. This practice ought by no means to be encouraged. The usual

defence for it is, that it is impossible now to identify the species to which the name was anciently applied. But it is certain that if any traveller will take the trouble to collect the vernacular names used by the modern Greeks and Italians for the Vertebrata and Mollusca of southern Europe, the meaning of the ancient names may in most cases be determined with the greatest precision. It has been well remarked that a Cretan fisher-boy is a far better commentator on Aristotle's 'History of Animals' than a British or German scholar. The use however of ancient names, *when correctly applied*, is most desirable, for "in framing scientific terms, the appropriation of old words is preferable to the formation of new ones*."

l. Adjective generic names.—The names of genera are, in all cases, essentially substantive, and hence adjective terms cannot be employed for them without doing violence to grammar. The generic names *Hians, Criniger, Cursorius, Nitidula*, &c. are examples of this incorrect usage.

m. Hybrid names.—Compound words, whose component parts are taken from two different languages, are great deformities in nomenclature, and naturalists should be especially guarded not to introduce any more such terms into zoology, which furnishes too many examples of them already. We have them compounded of Greek and Latin, as *Dendrofalco, Gymnocorvus, Monoculus, Arborophila, flavigaster*; Greek and French, as *Jacamaralcyon, Jacamerops*; and Greek and English, as *Bullockoides, Gilbertsocrinites.*

n. Names closely resembling other names already used.—By Rule 10 it was laid down, that when a name is introduced which is *identical* with one previously used, the later one should be changed. Some authors have extended the same principle to cases where the later name, when correctly written, only approaches in form, without wholly coinciding with the earlier. We do not, however, think it advisable to make this law imperative, first, because of the vast extent of our nomenclature, which renders it highly difficult to find a name which shall not bear more or less resemblance in sound to some other; and, secondly, because of the impossibility of fixing a limit to the degree of approximation beyond which such a law should cease to operate. We content ourselves, therefore, with putting forth this proposition merely as a recommendation to naturalists, in selecting generic names, to avoid such as too closely approximate words already adopted. So with respect to species, the judicious naturalist will aim at variety of designation, and will not, for example, call a species *virens* or *virescens* in a genus which already possesses a *viridis.*

o. Corrupted words.—In the construction of compound Latin words, there are certain grammatical rules which have been known and acted on for two thousand years, and which a naturalist is bound to acquaint himself with before he tries his skill in coining zoological terms. One of the chief of these rules is, that in compounding words all the radical or essential parts of the constituent members must be retained, and no change made except in the variable terminations. But several generic names have been lately introduced which run counter to this rule, and form most unsightly objects to all who are conversant with the spirit of the Latin language. A name made up of the first half of one word and the last half of another, is as deformed a monster in nomenclature as a Mermaid or a Centaur would be in zoology; yet we find examples in the names *Corcorax* (from *Corvus* and *Pyrrhocorax*), *Cypsnagra*

* Whewell, Phil. Ind. Sc. v. i. p. lxvii.

(from *Cypselus* and *Tanagra*), *Merulaxis* (*Merula* and *Synallaxis*), *Loxigilla* (*Loxia* and *Fringilla*), &c. In other cases, where the *commencement* of both the simple words is retained in the compound, a fault is still committed by cutting off too much of the radical and vital portions, as is the case in *Bucorvus* (from *Buceros* and *Corvus*), *Ninox* (*Nisus* and *Noctua*), &c.

p. Nonsense names.—Some authors having found difficulty in selecting generic names which have not been used before, have adopted the plan of coining words at random without any derivation or meaning whatever. The following are examples: *Viralva, Xema, Azeca, Assiminia, Quedius, Spisula.* To the same class we may refer *anagrams* of other generic names, as *Dacelo* and *Cedola* of *Alcedo*, *Zapornia* of *Porzana*, &c. Such verbal trifling as this is in very bad taste, and is especially calculated to bring the science into contempt. It finds no precedent in the Augustan age of Latin, but can be compared only to the puerile quibblings of the middle ages. It is contrary to the genius of all languages, which appear never to produce new words by spontaneous generation, but always to derive them from some other source, however distant or obscure. And it is peculiarly annoying to the etymologist, who after seeking in vain through the vast storehouses of human language for the parentage of such words, discovers at last that he has been pursuing an *ignis fatuus.*

q. Names previously cancelled by the operation of § 6.—Some authors consider that when a name has been reduced to a synonym by the operations of the laws of priority, they are then at liberty to apply it at pleasure to any new group which may be in want of a name. We consider, however, that when a word has once been proposed in a given sense, and has afterwards sunk into a synonym, it is far better to lay it aside for ever than to run the risk of making confusion by re-issuing it with a new meaning attached.

r. Specific names raised into generic.—It has sometimes been the practice in subdividing an old genus to give to the lesser genera so formed, the names of their respective typical species. Our Rule 13 authorizes the forming a new specific name in such cases; but we further wish to state our objections to the practice altogether. Considering as we do that the original specific names should as far as possible be held sacred, both on the grounds of justice to their authors and of practical convenience to naturalists, we would strongly dissuade from the *further continuance* of a practice which is gratuitous in itself, and which involves the necessity of altering long-established specific names.

We have now pointed out the principal rocks and shoals which lie in the path of the nomenclator; and it will be seen that the navigation through them is by no means easy. The task of constructing a language which shall supply the demands of scientific accuracy on the one hand, and of literary elegance on the other, is not to be inconsiderately undertaken by unqualified persons. Our nomenclature presents but too many flaws and inelegancies already, and as the stern law of priority forbids their removal, it follows that they must remain as monuments of the bad taste or bad scholarship of their authors to the latest ages in which zoology shall be studied.

[*Families to end in* idæ, *and Subfamilies in* inæ.]

The practice suggested in the following proposition has been adopted by many recent authors, and its simplicity and convenience is so great that we strongly recommend its universal use.

§ B. It is recommended that the assemblages of genera termed *families* should be uniformly named by adding the termination *idæ* to

the name of the earliest known, or most typically characterized genus in them; and that their subdivisions, termed *subfamilies*, should be similarly constructed, with the termination *inæ*.

These words are formed by changing the last syllable of the genitive case into *idæ* or *inæ*, as *Strix, Strigis, Strigidæ, Buceros, Bucerotis, Bucerotidæ*, not *Strixidæ, Buceridæ*.

[*Specific names to be written with a small initial.*]

A convenient *memoria technica* may be effected by adopting our next proposition. It has been usual, when the titles of species are derived from proper names, to write them with a capital letter, and hence when the specific name is used alone it is liable to be occasionally mistaken for the title of a genus. But if the titles of *species* were *invariably* written with a *small* initial, and those of *genera* with a *capital*, the eye would at once distinguish the rank of the group referred to, and a possible source of error would be avoided. It should be further remembered that all species are *equal*, and should therefore be written all *alike*. We suggest, then, that

§ C. Specific names should *always* be written with a small initial letter, even when derived from persons or places, and generic names should be always written with a capital.

[*The authority for a species, exclusive of the genus, to be followed by a distinctive expression.*]

The systematic names of zoology being still far from that state of fixity which is the ultimate aim of the science, it is frequently necessary for correct indication to append to them the name of the person on whose authority they have been proposed. When the same person is authority both for the specific and generic name, the case is very simple; but when the specific name of one author is annexed to the generic name of another, some difficulty occurs. For example, the *Muscicapa crinita* of Linnæus belongs to the modern genus *Tyrannus* of Vieillot; but Swainson was the first to apply the specific name of Linnæus to the generic one of Vieillot. The question now arises, Whoso authority is to be quoted for the name *Tyrannus crinitus?* The expression *Tyrannus crinitus*, Lin., would imply what is untrue, for Linnæus did not use the term *Tyrannus*; and *Tyrannus crinitus*, Vieill., is equally incorrect, for Vieillot did not adopt the name *crinitus*. If we call it *Tyrannus crinitus*, Sw., it would imply that Swainson was the first to describe the species, and Linnæus would be robbed of his due credit. If we term it *Tyrannus*, Vieill., *crinitus*, Lin , we use a form which, though expressing the facts correctly, and therefore not without advantage in particular cases where great exactness is required, is yet too lengthy and inconvenient to be used with ease and rapidity. Of the three persons concerned with the construction of a binomial title in the case before us, we conceive that the author who *first* describes and names a species which forms the groundwork of later generalizations, possesses a higher claim to have his name recorded than he who afterwards defines a genus which is found to embrace that species, or who may be the mere accidental means of bringing the generic and specific names into contact. By giving the authority for the *specific* name in preference to all others, the inquirer is referred *directly* to the original description, habitat, &c. of the species, and is at the same time reminded of the date of its discovery; while genera, being less numerous than species, may be carried in the memory, or

referred to in systematic works without the necessity of perpetually quoting their authorities. The most simple mode then for ordinary use seems to be to append to the original authority for the species, when not applying to the genus also, some distinctive mark, such as (*sp.*) implying an exclusive reference to the *specific* name, as *Tyrannus crinitus*, Lin. (*sp.*), and to omit this expression when the same authority attaches to both genus and species, as *Ostrea edulis*, Lin.* Therefore,

§ D. It is recommended that the authority for a specific name, *when not applying to the generic name also,* should be followed by the distinctive expression (*sp.*).

[*New genera and species to be defined* amply *and* publicly.]

A large proportion of the complicated mass of synonyms which has now become the opprobrium of zoology, has originated either from the slovenly and imperfect manner in which species and groups have been originally defined, or from their definitions having been inserted in obscure local publications which have never obtained an extensive circulation. Therefore, although under § 12, we have conceded that mere insertion in a printed book is sufficient for *publication*, yet we would strongly advise the authors of new groups always to give in the first instance a full and accurate definition of their characters, and to insert the same in such periodical or other works as are likely to obtain an immediate and extensive circulation. To state this briefly,

§ E. It is recommended that new genera or species be *amply* defined, and *extensively* circulated in the first instance.

[*The names to be given to subdivisions of genera to agree in gender with the original genus.*]

In order to preserve specific names as far as possible in an unaltered form, whatever may be the changes which the genera to which they are referred may undergo, it is desirable, when it can be done with propriety, to make the new subdivisions of genera agree *in gender* with the old groups from which they are formed. This recommendation does not however authorize the changing the gender or termination of a genus already established. In brief,

§ F. It is recommended that in subdividing an old genus in future, the names given to the subdivisions should agree in gender with that of the original group.

[*Etymologies and types of new genera to be stated.*]

It is obvious that the names of genera would in general be far more carefully constructed, and their definitions would be rendered more exact, if authors would adopt the following suggestion :—

§ G. It is recommended that in defining new genera the etymology of the name should be always stated, and that one species should be invariably selected as a type or standard of reference.

In concluding this outline of a scheme for the rectification of zoological nomenclature, we have only to remark, that almost the whole of the propositions contained in it may be applied with equal correctness to the sister science of botany. We have preferred, however, in this essay to limit our views

* The expression *Tyrannus crinitus* (Lin.) would perhaps be preferable from its greater brevity.

to zoology, both for the sake of rendering the question less complex, and because we conceive that the botanical nomenclature of the present day stands in much less need of distinct enactment than the zoological. The admirable rules laid down by Linnæus, Smith, Decandolle, and other botanists (to which, no less than to the works of Fabricius, Illiger, Vigors, Swainson, and other zoologists, we have been much indebted in preparing the present document), have always exercised a beneficial influence over their disciples. Hence the language of botany has attained a more perfect and stable condition than that of zoology; and if this attempt at reformation may have the effect of advancing zoological nomenclature beyond its present backward and abnormal state, the wishes of its promoters will be fully attained.

(Signed)	H. E. Strickland.	J. S. Henslow.
June 27, 1842.	John Phillips.	W. E. Shuckard.
	John Richardson.	G. R. Waterhouse.
	Richard Owen.	W. Yarrell.
	Leonard Jenyns.	C. Darwin.
	W. J. Broderip.	J. O. Westwood.

XVIII. *On the Geological Structure of the Ural Mountains.* *By* Roderick Impey Murchison, *Esq., F.R.S., Pres. G.S.,* M. E. de Verneuil, *and* Count A. von Keyserling*.

A SHORT introduction explains, that although the true geological relations of the rocks which constitute these mountains were previously little known, the Russians had become well acquainted with their mineral wealth and lithological structure. The skill and energy with which the mines have been worked having been adverted to, the authors dwell with pleasure upon the facilities which the Imperial Government afforded them by the instructions conveyed to all the mining establishments by the orders of Count Cancrine and the arrangements of Gen. Tcheffkine. They also acknowledge the advantages they derived from the co-operation of many officers at the different stations or zavods, several of whom prepared maps for their use†. They further express their obligations to many individual proprietors, and notably to M. Anatole Demidof, and the Prince Butera, for their very hospitable reception at the zavods of Nijny Tagilsk and Bissersk. They then proceed to state, that without the small general map recently published by Baron A. von Humboldt and his associates, the objects of the journey could not have been so well attained. These objects were, to reunite the various frag-

* From the Proceedings of the Geological Society, vol. iii. p. 742; being an abstract of a memoir read before the Society on the 18th of May, 1842. On the geology of Russia, see also pres. vol. p. 71, note.

† Among these officers allusion in this brief notice can only be made to those in command, viz. Gen. Glinka, Commander-in-chief at Ekaterinburg; Col. Völkner, formerly at Perm; Col. Protassof at Bogoslofsk, who first explored the districts north of that station; Col. Tchaikofski of Ekaterinburg, and Col. Galahofski of Turinsk.

ments of the Ural chain, to show of what sedimentary masses it was originally composed, and to explain by what agency the strata have been dislocated and altered. In the latter respect they are aware that their labours have to a great extent been anticipated by the researches of Baron Humboldt, and his companions M. G. Rose and M. Ehrenberg, as well as by their predecessors Colonel Helmersen and M. Hoffmann *, and various officers of the Imperial School of Mines †.

Moving in two parties and upon separate but parallel lines of research, the authors examined both flanks of the chain simultaneously, their force being brought together at the chief establishments by mutual converging traverses; and thus, in less than three months, they acquired a general knowledge of the chain from Bogoslofsk on the north to Orsk and Orenburg on the south, a distance of about 550 miles. It is not pretended that this knowledge is precise in relation to the mineral structure of the mining tracts; as such details either have been or will be worked out by Russian engineers. The authors merely hope to have succeeded in giving *an unity of geological composition* to the chain, so that the age of the chief masses may be effectively compared with the unaltered deposits of the plains of Russia, and by this means with the geological succession of sedimentary deposits already established in Europe.

Physical Features.—Referring to Capt. Strajefski for his account of the northernmost and uncolonized part of the chain, which he explored amid great privations to 65° N. lat., the physical geography of the civilized portion is briefly sketched, and the chief altitudes, as determined by Colonel Helmersen, are given. The general bearing of the chain, as well known, trends from north to south. Ekaterinburg, the chief town, is situated on the eastern side of the only very low depression in the range, from which point this dividing crest between Europe and Asia rises both to the north and south, and attains altitudes occasionally of 2500 feet. The northern Ural, formerly occupied by Voguls, who still live in the wildernesses north of 61 degrees, is inhospitable in climate, and is chiefly occupied by dense forests, through which the rocks of the central water-shed are perceptible only at intervals. This monotony, however, is enlivened by knots of mountains which rise up on the sides of the parting ridge, and overtop it. Such are the Katch Kanar, the Pawdinskoi Kamen, near Bogoslofsk, 2784 English feet, and the Konjakofski Kamen, to the north of the same places, about 5700 feet above the sea ‡. Whilst the North Ural (or that north of Ekaterinburg) has one persistent direction with some lower flanking ridges parallel to the chief

* See various works on given districts of the Ural mountains by officers of the Imperial School of Mines.

† These works are referred to and ably condensed in a Russian work by Prof. Stshurofski of Moscow.

‡ This mountain was once estimated to have an altitude of 8000 or 9000 feet, but by the trigonometrical observations of Fedoroff and the barometrical calculations of Kupffer, it has been ascertained that it cannot exceed 5280 Paris feet above the sea. It was upon this point of the range that the authors saw much snow in the month of July.

one, the whole not occupying a breadth of more than from 45 to 70 miles, the South Ural, *i. e.* to the south of the mountain *Jurma**, expands to much greater width, branching off into fan-shaped ridges, which trend to the south and to the east and west of that point. In this region, however, as in the north, the water crest or *Ural-tau* preserves a north and south direction, varying in height from 1800 to 2500 feet, whilst the broken ridges on its western flanks, such as the Taganai near Zlataoust, rise to 3800 English feet, and the Iremel to about 5136 English feet above the sea.

From its configuration, and also from its latitude, the South Ural, inhabited by Baschkirs, is infinitely more picturesque than the North Ural; but, with the exception of the environs of Miask and Zlataoust, it is much less rich in mines than the North Ural.

Geological Structure.—The Ural mountains consist of ancient sedimentary strata, which, in the central parts of the chain and on its eastern or Siberian flank, are for the most part in a highly metamorphic condition; also of various rocks of igneous or intrusive origin.

Owing to the eruption of the latter at numberless points and along great zones of fissure parallel to the axis of the chain, the ancient deposits are so dismembered and altered, that it is at intervals only they can be deciphered. The rocks are described in descending order, or from the flanks to the centre of the chain.

Carboniferous System.—By examining these mountains from their western slopes, where igneous rocks are comparatively scarce, the authors, in consequence of their knnwledge of the palæozoic strata of western Europe and Russia in Europe, had no great difficulty in reading off the true order of succession on the banks of the Tchussovaya, Serebrianka, and other transversely-flowing streams. In the first place, the beds of sandstone, conglomerate and calcareous flags alluded to in the former memoir† are seen to rise from beneath the Permian deposits, and containing in some parts thin courses of coal, and in others coal-plants, Goniatites and certain fossils, represent the upper members of the carboniferous system. These strata are succeeded by a thick formatinn of hard quartzose grit and sandstone, very much resembling the millstone grit of some parts of England. Beneath this is the carboniferous or mountain limestone, properly so called, of English geologists, and which is recognised by containing many of the same typical fossils as in England and other parts of Russia. Thus defined, the carboniferous system occupies, on the western side of the chain, a very wide zone, which to the south of Kongur is expanded into a large trough extending beyond the parallel of 55° N. lat., and flanked upon the west and east by upcasts of the limestone, it contains in its centre the great undulations of the grits and conglomerates just spoken of.

A third and less prolonged, but most remarkable zone of this limestone appears in four insulated hills extending north and south of Sterlitamak, and perfectly parallel to the chain. It is in the

* About 3000 English feet above the sea. All these heights are taken from Colonel Helmersen and M. Hoffmann.

† See pres. vol. p. 64.

southern prolongation of this line of upheaval that the Permian red sandstones and limestones of Gre-beni and Orenburg are thrown into anticlinal positions, the axis of which is also parallel to that of the adjacent older rocks. For reasons hereafter adduced, it is inferred that this anticlinal was formed subsequent to the chief elevation of the chain.

Devonian Limestones, &c.—The Devonian rocks of the North Ural are seen on the banks of the Tchussovaya in the form of limestones, grits and schists, which pass into the lower carboniferous limestone, the latter being always in highly inclined, sometimes in very contorted and even inverted positions, the younger rocks dipping under the older. These Devonian limestones much resemble, in their dark colour and subcrystalline aspect, those of South Devon in England, and they contain fossils characteristic of this division both in the British Isles, in Belgium, Prussia and the Eifel; but though perfectly identified both by position and contents with the Devonian rocks of the flat regions of Russia, the Uralian strata are as dissimilar from them in external aspect as the rocks of the same age in Devonshire are from the old red sandstone of the north of Scotland and of Herefordshire or Brecon in England. Nor are these Devonian rocks on the western flanks of the Ural separated from the lower carboniferous limestone by any band of sandstone and coal as in the northern parts of Russia in Europe, but the grey limestone of the overlying group is at once succeeded by the dark limestone of the other, both undergoing the same flexures, and both forming parts of one great palæozoic series.

In their prolongation to the south, the limestones of this Devonian group thin out and inosculate with a considerable development of red sandstone, grit, fine conglomerate and schist, in some parts resembling the old red sandstone of the Highlands. A peculiar mineral character of these Devonian limestones is, that they retain their *black* colour even when in the state of dolomite.

Silurian Rocks.—The schists and flagstones which underlie these limestones are considered to be of Silurian age; with these strata are associated beds of limestone for the most part concretionary, and which are well developed on the banks of the Serebrianka from the zavod of Serebriansk to near its mouth. Among the predominant fossils of this group and amid numerous corals, the *Terebratula prisca* (*Atrypa affinis*, Sil. Syst.) is clustered together in great masses, as in the Ludlow rocks of England, and with it are associated the remarkable *Leptæna Uralensis* and other new species. The same descending sequence cannot be so well seen in many parts of the North Ural, as on the banks of the Serebrianka.

Immediately, however, to the east of the water-shed (viz. from Bogoslofsk to Nijny Tagilsk and Neviansk), broken masses of limestone, insulated amid plutonic rocks, are charged with large Pentameri, closely approaching to the *Pentamerus Knightii* of the upper Silurian rocks, and associated with Orthis, Terebratula and other fossils, which, from collections sent to him, M. de Buch has classed as Silurian forms (see Beiträge der Geb. Form. in Russ-

land. Von L. Von Buch. 1840). Although then the clear stratigraphical sequence is interrupted, there is no doubt that the equivalents, at least of the upper members of the Silurian rocks, exist in these mountains; and in tracing such into the South Ural, particularly by a transverse section from Verch-Uralsk to Sterlitamak, the authors convinced themselves, from the presence of Orthidæ, Pentameri, &c., that where not much interfered with by intrusive rocks, the central deposits of the chain (usually however in the state of slate and quartz rock) belong to the Silurian system, and probably to its lowest divisions.

The symmetry which is developed on the western side of the water-shed is almost obliterated to the east by the greater frequency of eruptive matter and the abundance of metamorphic and metalliferous rocks. Thus in passing eastwards into Siberia on any parallel, from Bogoslofsk, Nijny Tagilsk, Ekaterinburg, Miask or Verch-Uralsk, no regular succession can be traced; as large zones of igneous and crystalline rocks intervene, and thus different members of the palæozoic series are met with upon the same strike. In some spots however, notwithstanding all this confusion, transitions can be traced from lower to higher formations. At Bogoslofsk, for example, a passage may be observed from Silurian to Devonian strata; and though all the formations are not in apposition to the east of Ekaterinburg, the section of the river Isset clearly shows, that, after various undulations, the Devonian limestones and schists on the west are succeeded on the east by true carboniferous limestone with large Producti, this latter deposit being in some instances based upon conglomerates and grit. Whilst this succession is exposed in a region penetrated by many points of eruptive trap and porphyry, the whole of the less altered group reposing on micaceous schists and other granitic rocks, a mass of Pentamerus limestone is thrown up in an insulated tract at a small distance; and as this limestone is quite dissimilar from any visible in the adjacent gorges of the Isset, where the Devonian and carboniferous limestones are fully developed, the authors conclude that it belongs to the Silurian epoch.

On the eastern flank of the southern Ural the ancient sedimentary rocks occur in great undulations. At Troitsk in the steppes of the Kirghis, or beyond a chain of granite separated from and parallel to the Ural (see map), Silurian and Devonian limestones occur, whilst at Cossatchi Datchi, close to the eastern flank of the Ural, there is a small basin of palæozoic rocks, the limestone of which is proved to be true carboniferous, by containing a vast profusion of fossils, many of which are common to the Waldaï limestones of Russia, and the mountain limestone of the British Isles and Belgium. In following southward the eastern slopes of the chain where they border on the river Ural, promontories of carboniferous limestone rise up in undulations, supporting troughs of the coarse carboniferous grits and conglomerates before alluded to; and on passing the axis of the chain between Orsk and Orenburg, where it dwindles to a small height, the same carboniferous group of lime-

stones, conglomerates and grits is thrown off to the west upon the face of the igneous rocks forming the Guberlinsk hills. In travelling westwards to Orenburg, particularly from the limestone hills of Gourmaya, the authors found a most instructive section, developing the ascending order from the great carboniferous limestone through the overlying grits, flagstone and calcareous grits with Goniatites, into the beds with gypsum, which form the base of the Permian system, the whole being distinctly overlaid by conformably inclined strata of cupriferous grits, red sandstone, shale and limestones containing fossils of the zechstein.

Upon the eastern flanks of the Ural, on the contrary, granitic and other igneous rocks rising (as before said) to the surface, that region is entirely void of all those strata which in Russia in Europe are interposed between the carboniferous and Jurassic systems. Beds belonging to the latter system have indeed been detected at two very widely distant localities, the one in 65° N. lat. by Capt. Strajefski, the other forming a plateau in the southernmost extremity of the chain north of Orsk, where they were first observed by Col. Helmersen*. It must however be observed, that the great mass of the chain is void of Jurassic strata, nor have its eastern flanks afforded any evidences of cretaceous or tertiary rocks, as identifiable by organic remains. From this last remark, the authors would except certain grits which occur in patches in the lower country of Siberia, notably at Kaltchedansk, east of Ekaterinburg. These grits, which are largely quarried for millstones, might almost be called "trachytic," as they resemble in composition some of the rough trachytes of Hungary, and like which they pass into an impure pitchstone grit. From the associated amber and beds of clay, it may however be inferred, that these rocks were formed under water, and that they owe the trachytic aspect to their having resulted from the detritus of the quartzose porphyries on which they repose. They are probably continuous masses of the grits described by G. Rose, near Verkhoturie. Sections on the river Isset explain these phænomena.

Igneous, Metamorphic and Metalliferous Rocks.—As it formed subordinate parts only of the objects of the authors, either to study the details of the metamorphism of the sedimentary strata produced by the intrusion of igneous rocks, or the associated simple minerals, the relations of both of which have been so elaborately described by Mons. G. Rose, this portion of their memoir is chiefly confined to a sketch of some striking phænomena of this class. No true granite appears in the higher mountains, the syenite which is seen at intervals being intimately allied to greenstone; and the latter, with its various modifications, is by far the most abundant of the intrusive rocks which appear on or along the immediate flanks of the Ural ridge†. Whenever these greenstones and traps rise to the

* The authors did not visit the last-mentioned spot, but, from the communication of their friend Colonel Helmersen, they have little doubt that this deposit is a fragment of the Jurassic range which they traced to the south and west of Orenburg.

† The granitic region is in Siberia, to the east of the Ural.

surface, the strata in their proximity are highly altered. Thus even when studied on a small scale ou the western flank of the mountains, as at the baths of the Zavod of Sergiefsk, the sandstone in contact is altered into quartz rock, and the limestone, so regularly bedded and full of fossils at a little distance, is converted into an amorphous, crystalline, splintery mass, charged with cross veins, and sulphureous saline waters flow from its base, the adjacent rocks being also much impregnated with iron ore. Similar but on a far grander scale are the phænomena of intrusion and metamorphism which are presented by the central axis of the Ural, and to a less extent by all the parallel ridges which flank it on the eastern or Siberian side.

The Ural-tau or crest is to a very great extent a wall of schist and quartz rock diversified by points of igneous rocks, and though of no great altitude, it is very remarkable that throughout 17 degrees of latitude this water-shed is not broken through by any great transverse valley. The Ural-tau marks, in fact, one long line or fissure of eruption. With the exception of the gold mines near Bissersk, on its west flank, all the gold alluvia of the chain occur on its eastern flank; and when it is stated that this circumstance is connected with the fact, that all the great masses of igneous rocks have been evolved on the eastern flank, it will at once be seen (as insisted upon so well by Humboldt) that there is an intimate connexion between the eruption of plutonic rocks and the formation of the gold mines [veins ?] whence the local alluvia have been derived. That this connexion exists in regard to other mineral veins, is also equally apparent in the Ural mountains; for with very rare exceptions, it is only on their eastern or eruptive side that copper veins, malachite, platinum and magnetic iron prevail*.

Without entering into all the lithological distinctions of the North Ural, they advert specially to the occurrence in the districts of Turinsk and Nijny Tagilsk of a stratified and regularly bedded porphyry, which they compare with the "Schaalstein" of German geologists, and which on the banks of the Kakwa and east of Bogoslofsk†, as in the Rhenish provinces, alternates with limestone strata of Devonian age. In the copper mines of Turinsk, the veins and masses of ore are shown to be intimately connected with the intrusion of greenstone, between a thick mass of which and the metalliferous veins is a garnet rock. This phænomenon is a counterpart to that formerly described by Professor Henslow in the Isle of Anglesea; whilst on the river Kakwa, the ordinary limestone (Devonian?) has been converted by a dyke of greenstone into white granular marble, in the same way as by the contact of syenite the lias limestone of the Isle of Sky has been changed.

The Katch-kanar mountain (lat. 58° 44′), which the authors visited by a little-frequented pass, is composed of augitic greenstone

* In sketching the chief relations of the plutonic and metamorphic rocks of the North Ural, much praise is given to a detailed geological map of the environs of Bogoslofsk by Capt. Karpinski, of the School of Mines, with a copy of which the authors were furnished.

† See Geol. Trans. vol. vi. pp. 246, 248.

and magnetic iron, the latter in so hard and crystalline a state that it is not worthy of extraction and manufacture. The most productive masses of magnetic iron are at the Government establishment of Mount Blagodat, and that of Nijny Tagilsk, belonging to M. Anatole Demidoff. In both these cases the ordinary varieties of iron occur in great masses, occasionally with chromate of iron*, in contact with rocks of igneous origin, in which serpentine, compact felspar, greenstone, porphyry, &c. are apparent. At Nijny Tagilsk the chief intrusive rock (greenstone) is coated by prodigious masses of the iron ore, which is worked in open quarries, and is most magnetic where it is in contact with greenstone. Copper ores also abound at this spot, and some of them are associated with Silurian limestone, often highly mineralized, but in which large Pentameri and other fossils are observed; also witb a bedded trappean rock or schaalstein, which is in parts highly cupriferous. It is from such ancient rocks that copper solutions are supposed to have flowed, in very remote periods, into the adjacent low countries on the west, then under the sea, and to have impregnated the sandstones and grits of Perm during their formation. The malachites of this place have long been celebrated, and, from their structure as well as their position, in cavities of the rock, they are supposed to have been formed by ancient stalactitic depositions. The ores of platinum, though hitherto found in alluvia only, always occur near the protrusive igneous rocks. Magnetic iron ore and copper ore are stated to occur at many other localities, and always under similar circumstances.

Gold Ores.—Though the great supply of gold which the Ural mountains afford, is derived from alluvia, the ore has been found in veins which are slightly worked at Berosofsk, near Ekaterinburg, and were formerly near Miask†. Wherever gold veins or gold alluvia have been discovered, the auriferous matter is flanked by rocks of intrusive origin, and these are very frequently serpentine. It has however been shown by Humboldt and Rose, who, in the first volume of their recent work, have described twenty-seven sites of gold alluvia in these mountains, that the auriferous detritus rests upon a great variety of rocks, viz. talcose, chloritic, siliceous, argillaceous schists and encrinite limestone, as well as upon granite, greenstone and serpentiue, though most frequently on the last-mentioned rock. An observation also of these authors is important, as bearing upon the relative date of the origin of gold, viz. that the veins containing it have been seen by them to cut through not only the schists and the beresite (according to them

* The largest masses of chromate of iron occur in the South Ural, near the mines of Polikofski, south of Miask, and from whence from 6000 to 7000 "pouds" per annum have recently been sent to Moscow.

† In the tracts around Miask aud Zlataoust the authors were most cordially and judiciously assisted by General Anosof, an officer highly distinguished for the metallurgic processes and the manufacture of small-arms which he directs. His assistant, Major Lissenko, who has prepared a mineralogical map of the surrounding country, was also kindly serviceable to them.

a felspathic granite), but also the serpentine; thus seeming to prove that the gold veins have resulted from one of the very last changes which have affected this region. (Rose, vol. i. p. 422.)

It is stated, that as the alluvia containing gold are purely of local origin, or derived from the adjacent hills, their accumulation can have no reference to the actual period, and present rivulets or waters, for the deposits lie at considerable heights above their beds, contain bones of mammoths, the extinct rhinoceros, and, in some instances, are even traceable over small ridges of intrusive and altered rocks from veins whence the detritus was doubtless derived, and accumulated in its present state at the period when the large mammals were destroyed. Numerous sections are given at Berosofsk, Soimanofski Zavod, and notably from the environs of Miask and Cossatchi Datchi, all of which tend to establish these views, as well as those of the alteration, mineralization and crystallization of the palæozoic strata by the intrusion of igneous matter, and prove that the alluvia were collected anterior to the existing epoch. Some of the gold alluvia are exclusively composed of carbouiferous limestone replete with fossils (Cossatchi Datchi).

In concluding this sketch of the Ural mountains, the authors advert to the remarkable fact, that all the superficial detritus is local, and that no large boulders or blocks transported from afar are visible either in the chain or in the low countries on its flanks; and they also state, that they nowhere observed among the higher portions of the mountains any traces of those scratches or polishings of the rock which are common in some parts of Europe, and which are supposed to have been produced by glacial action.

Original maps and sections of the districts around the mining establishments of Bogoslofsk, Turinsk and Blagod at Ekaterinburg, Soimanofsk, Zlataoust and Miask, prepared by the officers of the Imperial School of Mines, were exhibited, as well as a map of the North Ural to 65° N. lat., drawn by Strajefski, together with a most elaborate geographical map of the South Ural, executed by orders of General Perovski, under the superintendence of the officers of the staff of his government, directed by General Rakosofski*. From all these documents and others published in the volumes of the 'Journal of the School of Mines,' combined with their own observations, the authors have coloured geologically the map of Humboldt, a reduction of the chief features of which will appear in a map now in progress, which will accompany their forthcoming work on Russia and the Ural mountains.

General Conclusions.—In greatly extending the knowledge which they had previously acquired, the survey of last year has enabled the authors to modify their earlier views concerning the equivalents of some of the strata of Russia in Europe. With respect to their former account of the great tripartite palæozoic series of beds which covers such large portions of Northern Russia, they have

* This map is illustrated by a description of the physical features of South Ural from the pen of M. Khanikof, which Mr. Murchison has communicated to the Royal Geographical Society of London.

nothing to retract. On the contrary, by adding to their previous lists a great number of typical organic remains well known in Western Europe, they are still more convinced of the accuracy of their first classification, and of the existence of large zones of Silurian, Devonian and carboniferous rocks, clearly separated from each other by their order and their imbedded fossils.

The newly discovered dome of Devonian rocks in the centre of European Russia is a feature of great importance, in explaining the difference between the mineral basin to the north and that to the south of it. The carboniferous system, the most widely extended deposit of Northern Russia, has now been subdivided into stages, each characterised by its fossils; and it has been clearly shown, that the most productive of the coal-bearing strata in the Russian empire, viz. those of the southern steppes, are associated with the mountain limestone; whilst the uppermost member of the system, or coal-measures, which is so rich in coal in Western Europe, if indeed it exists, is nearly unproductive in Russia.

The next great group of rocks in ascending order, is that which has been elaborated in considerable detail under the name of the Permian system, and which, as already shown, is to be considered as a vastly expanded equivalent of the zechstein and associated beds of Germany and the magnesian limestone of the British Isles. This system is rendered much more important by its fossil contents in Russia than by any remains which have been discovered in it in other parts of Europe; for not only does it contain, like the zechstein of Germany and the magnesian limestone of England, the remains of thecodont saurians and certain fishes (Palæonisci), but also a fauna much more copious in other classes, and a flora infinitely more rich than any which had been previously made known as pertaining to rocks of this age. This flora is shown to be of intermediate characters between that of the carboniferous system and the plants which have been published as typical of the trias.

The Permian system is also of high interest in setting before us the example of wide accumulations impregnated throughout great thicknesses with copper, and as this matter has manifestly been derived from the mineral masses of the adjacent Ural, so is it inferred that these mountains constituted dry land on which the plants in question grew, and that the latter having been washed down into these Permian deposits were there rendered the nuclei of the copper ores which are arranged around them. The thin layer of kupfer schiefer of Germany may be considered as the miniature representative of this great metalliferous deposit, whilst in its large masses of gypsum, the Permian deposits exceed even the zechstein on the south of the Hartz*. The Jurassic system of Russia reposes on the Permian and older rocks without clear evidence of the existence of any part of the Triassic group, there being no traces of the muschelkalk limestone nor yet of the keuper; and it is with doubt even that the authors

* The authors use the term "Permian" in reference to Russian deposits *only*, and they by no means seek to interfere with the general use of the word "Zechstein," which has been so long sanctioned by the highest German authorities.

refer any portion of certain red strata which partly overlie the Permian rocks to the "bunter sandstein," or new red sandstone of geologists.

True lias has not yet been seen, but the Jurassic system is clearly divisible into upper and lower formations, and is followed by the cretaceous and tertiary systems, the latter including eocene, miocene and pliocene shells, and all these groups are copiously developed and clearly recognisable by their respective mollusca.

The geological survey of the flat regions of Russia, add the authors, in affording the best proof which has yet been obtained in any part of the world of *the same extent*, that distinct forms of animal life were successively created and entombed in each succeeding deposit, has also demonstrated that the successive obliteration of these classes was not caused by the outburst of contiguous plutonic rocks or great physical disturbances of the strata; for in this region, as large as the whole of those districts of the continent of Europe where geology has been most studied, no intrusive rocks are visible, and the wide-spread formations from the Silurian to the youngest tertiary, which must have occupied so vast a lapse of time in their accumulation, as well as the beds of retired modern seas, all repose conformably upon each other. And yet with this regular sequence throughout so vast a series and the absence of any great ruptures, the contents of each succeeding system of older strata are as clearly separable from each other as in those parts of the world where younger rocks are incumbent on the uplifted edges of those which had been previously dislocated.

But whilst they offer no traces of great and violent upheavals, the horizontal rocks of Russia bespeak most clearly that their surface has been so far acted upon by elevatory or subsiding movements, that in some tracts great thicknesses of strata are omitted. Bounded as this large geographical basin has been in remote epochs by the plutonic eruptions of Lapland and Sweden on the north, of the Ural on the east, of the granitic steppe on the south, and of the trappean rocks of Poland and Silesia on the west, it is possible, however, that the changes which were evolved in these regions may have affected and influenced the distribution of animal life in the great Muscovite depression which they surrounded. As every geological phænomenon in the strata of the plains of Russia indicates a submarine succession, so does the surface announce the same conditions. In the far northern districts the bottom of the Arctic Sea has been shown, by the presence of many existing species of shells, to have once extended over a wide tract of land, now 150 or 200 feet above the sea-level; and in the south-west it is known by like proofs that the Caspian once covered still wider districts of the steppes. Again, the authors have endeavoured to show that the mammoth alluvia, the boulders of the North and the black earth of Central and Southern Russia, have all been accumulated *under water*.

In reference to the question of the transport of the northern blocks, the authors conceive that their last survey has tended very materially to strengthen the opinions which they previously expressed, that such materials were carried to their present positions

by floating icebergs liberated from ancient glaciers in Scandinavia and Lapland, at a period when Russia in Europe was submerged. The examination of the Ural has in the meantime convinced them of the utter inapplicability of a terrestrial glacial theory even to all mountainous tracts of the earth; for these mountains, the peaks of which rise to upwards of 5000 feet above the sea, though situated in so cold a climate as to be now covered with snow during eight months in the year (and some peaks are never uncovered), show none of those signs insisted on by glacialists, of their having been at any period the residence of permanent glaciers. With the total absence of such proofs, so it is a striking confirmation of the connexion between glaciers and the blocks which in Russia in Europe are supposed to have been floated from Scandinavia and Lapland, that the flanks of the Ural chain and the adjacent plains are entirely void of all such far-transported detritus*.

XIX. *Proceedings of Learned Societies.*

ROYAL IRISH ACADEMY.

[Continued from vol. xxii. p. 495.]

Nov. 30, 1841. — THE following communication "On the Compound Nature of Nitrogen," by George J. Knox, Esq., was read.

Soon after the discovery of the bases of the alkalies and earths by Sir Humphry Davy, the compound nature of nitrogen began to be a subject of discussion amongst chemists; but the arguments in favour of this supposition, deduced principally from the nature of the ammoniacal amalgam, led to no satisfactory physical results.

The experiments of Sir Humphry Davy on the ammoniacal nitruret of potassium, and those of Despretz and Grove† on the compounds of nitrogen with iron, copper, &c., have shown that the metals singly (even when aided by the most powerful electrical induction) have not the power of decomposing nitrogen. There is one experiment, however, by Sir Humphry Davy, from which one might deduce its compound nature.

Upon heating ammonia-nitruret of potassium in an iron tube, he obtained more hydrogen, and less nitrogen, than the ammonia ought to have given.

Again: on mixing this substance with a greater proportion of potassium, he obtained still more hydrogen, and less nitrogen; whereas, on heating the same substance in a tube of *platinum*, the potassium alloyed with the platinum, and the ammonia was given off almost entirely undecomposed.

How can these experiments be explained except upon the suppo-

* The authors announced that the geological map of Russia and the Ural, as taken from their larger documents, would be published in a short time, and that their work descriptive of all the phænomena alluded to in these notices would be prepared in the ensuing [1842–43] winter.

[† Prof. Grove's paper here referred to will be found in Phil. Mag. S. 3. vol. xix. p. 97; and a notice of M. Despretz's experiments in Phil. Mag. S. 2. vol. vi. p. 147.—Edit.]

sition that the potassium and the iron had *conjointly* decomposed the nitrogen? The latest experiments which bear upon this subject, and from which I received the idea which led me to this investigation, are those of Dr. Brown* "upon the conversion of carbon into silicon," an explanation of phænomena which appears to me most unreasonable, and contrary to all chemical analogy; whilst the supposition of the carbon having reduced the nitrogen is not only a simple but an unavoidable conclusion to arrive at, if nitrogen be a compound substance. To determine, by experiment, the correctness or incorrectness of this idea, it were only necessary to reduce nitrogen by some other substance than charcoal; and should silica result from its decomposition, the problem might be considered to be solved.

Exp. I.—A considerable quantity of ammonia-nitruret of potassium was formed, by passing ammonia over potassium heated in an iron tube; the part which had not been in contact with the tube, having been examined for silica, contained none.

Exp. II.—Ammonia was passed for several hours over pure iron, heated to a dull red heat; examined for silica, it contained none.

Exp. III.—Ammonia-nitruret of potassium was heated with pure iron in an iron crucible, for one half hour, over a large Rose's lamp; the contents of the crucible, on examination, gave silicon and silica, the weight of which was not registered, as it might have been said to have derived a portion of silica from the inner surface of the crucible.

Exp. IV.—Twenty grains of ammonia-nitruret of potassium were heated with twenty grains of pure iron in the *same* iron vessel for one half hour; when treated with nitric and muriatic acids there remained insoluble a small quantity of a brownish colour, which, when fused with carbonate of potash, gave of silica 0·10. The solution, supersaturated with potash, filtered, neutralized, evaporated to dryness, gave of silica 1·450; sum total of silica 1·550.

From these experiments, together with those of Sir Humphry Davy mentioned above, one might infer that nitrogen is either a compound of silicon and hydrogen, or of silicon, hydrogen, and oxygen; to determine which, synthetically, a current of dry muriatic acid gas was passed over siliciuret of potassium (formed by heating silica with potassium), placed in a bent tube of Bohemian glass, the extremity of which dipped into a cup of mercury, lying on the bottom of a vessel filled with water. The atmospheric air had been previously expelled from the apparatus by a current of hydrogen.

The gases insoluble in water having been collected, were found, on examination, to be hydrogen and nitrogen, the relative proportions of which varied in different experiments.

In two experiments the proportions of hydrogen to nitrogen were four of the former to one of the latter.

In a third experiment, as six of hydrogen to one of nitrogen.

In a fourth, as five of hydrogen to four of nitrogen.

Observation.—White fumes appeared occasionally in the tube, indicating the presence of muriate of ammonia.

[* See Phil. Mag. S. 3. vol. xix. p. 295, 388; vol. xx. p. 24.—EDIT.]

Professor Lloyd exhibited a specimen of rock from Terre Adele.

Professor MacCullagh communicated to the Academy a very simple geometrical rule, which gives the solution of the problem of *total reflexion*, for ordinary media and for uniaxal crystals.

First, let the total reflexion take place at the common surface of two ordinary media, as between glass and air, and let it be proposed to determine the incident and reflected vibrations, when the refracted vibration is known. It is to be observed, that the refracted vibration (which is in general elliptical) cannot be arbitrarily assumed; for, as may be inferred from what has been already stated (Proceedings of the Academy, vol. ii. p. 102. Phil. Mag. S. 3. vol. xxi. p. 232), it must be always similar to the section of a certain cylinder, the sides of which are perpendicular to the plane of incidence, and the base of which is an ellipse lying in that plane and having its major axis perpendicular to the reflecting surface, the ratio of the major to the minor axis being that of unity to the constant r. The value of r, as determined by the general rule given in the place just referred to, is

$$r = \sqrt{1 - \frac{1}{n^2 \sin^2 i}},$$

where i is the angle of incidence, and n the index of refraction out of the rarer into the denser medium. The ellipse is greatest for a particle at the common surface of the media; and for a particle situated in the rarer medium, at the distance z from that surface, its linear dimensions are proportional to the quantity $e^{-\frac{2\pi r z}{\lambda}}$; so that for a very small value of z the refracted vibration becomes insensible.

Now, taking any plane section of the aforesaid cylinder to represent the refracted vibration for a particle situated at the common surface of the two media, let O P and O Q be the semiaxes of the section, and let them be drawn, with their proper lengths and directions, from the point of incidence O; through which point also let two planes be drawn to represent the incident and reflected waves. Then conceive a plane passing through the semiaxis O P, and intersecting the two wave-planes, to revolve until it comes into the position where the semiaxis makes equal angles with the two intersections; and in this position let the intersections be made the sides of a parallelogram, of which the semiaxis O P is the diagonal. Let O A and O A′, which are of course equal in length, denote these two sides. Make a similar construction for the other semiaxis O Q, and let O B, O B′, which are also equal, denote the two sides of the corresponding parallelogram. Then will the incident vibration be represented by the ellipse of which O A and O B are conjugate semidiameters, and the reflected vibration by the ellipse of which O A′ and O B′ are conjugate semidiameters. And the correspondence of *phase* in describing the three ellipses will be such that the points A, A′, P will be simultaneous positions, as also the points B, B′, Q.

The same construction precisely will answer for the case of total reflexion at the surface of a uniaxal crystal, which is covered with a

fluid of greater refractive power than itself. It is to be applied successively to the ordinary and extraordinary refracted vibrations, and we thus get the *uniradial* incident and reflected vibrations, or rather the ellipses which are similar to them. And as any incident vibration may be resolved into two which shall be similar to the uniradial ones, we can find the reflected vibration which corresponds to it, by compounding the uniradial reflected vibrations.

It may be well to mention that, in a uniaxal crystal, the plane of the extraordinary refracted vibration is always perpendicular to the axis, and therefore the ellipse in which the vibration is performed may be easily determined by the principles already laid down. The plane of the ordinary vibration has no fixed position in the crystal; but if we conceive the auxiliary quantities ξ_1, η_1, ζ_1 (Phil. Mag. S. 3. vol. xxi. p. 230) to be compounded into an ellipse (as if they were displacements), the plane of this auxiliary ellipse will be perpendicular to the axis of the crystal.

Whether the preceding very simple construction, for finding the incident and reflected vibrations by means of the refracted vibration, extends also to the case of *biaxal* crystals, is a point which has not yet been determined, on account of the complicated operations to which the investigation leads, at least when attempted in any way that obviously suggests itself.

A paper was read by William Roberts, Esq., F.T.C.D., "On the Rectification of Lemniscates and other Curves."

Let a curve be traced out by the feet of perpendiculars dropped from a fixed origin upon the tangents to a given curve: and from this new curve, let another be derived by a similar construction, and so on. Also let a curve be imagined which is constantly touched by perpendiculars to the radii vectores of the given curve, drawn at the points where it is met by these radii, and from this let another be derived by a similar mode of generation, and so on.

Then if s^n denote the arc of the curve which is nth in order in the former series, and s_{-n} that of the nth in the latter, we shall have

$$d s_{\pm n} = \frac{\pm n r \frac{d^2 \omega}{d r^2} + (1 \pm n) \frac{d \omega}{d r} + r^2 \frac{d \omega^3}{d r^3}}{\left(1 + r^2 \frac{d \omega^2}{d r^2}\right)^{\frac{\pm n + 1}{2}}} \left(r \frac{d \omega}{d r}\right)^{\pm n - 1} r \, d r,$$

$F(r, \omega) = 0$ being the polar equation of the given curve.

It is convenient to distinguish the curves of the two series by calling those of the former *positive*, and those of the latter *negative*; we may also generally denote their polar coordinates by the symbols $r_{n \pm}$, $\omega_{n \pm}$.

If the given curve, which may be denominated the base of either system, be an ellipse whose centre is the origin, it will be found, by applying the above formula, that the negative curves will in general have their arcs expressible by elliptic integrals of the first and second kinds, whose modulus is the eccentricity of the base-ellipse. The arc of the first will involve only a function of the first kind: a result

which has been given by Mr. Talbot, in a letter addressed to M. Gergonne, and inserted in the *Annales des Mathématiques*, tom. xiv. p.380.

A function of the third kind, with a circular parameter $-1+b^4$, where b is the semiaxis minor of the ellipse, its semiaxis major being unity, and the modulus of which is the eccentricity, enters into the arcs of all the positive curves; and their general rectification depends only on that of the ellipse, and of the first derived, both positive and negative.

The quadrants of the ellipse, and of the first two curves, positive and negative, are connected by the following relation: —

$$(S_{-1}+S_1)\,S_{-1}=(3\,S-S_{-2})\,(2\,S-S_2).$$

It is worthy of notice, that if the eccentricity be $\frac{\sqrt{5}-1}{2}$, the functions of the third kind disappear, and the rectification of both series depends only on that of the ellipse and of the first negative curve.

If the base curve be a hyperbola, whose centre is the origin, the arcs of all the curves of the negative series will depend only on elliptic functions of the first and second kinds. But the general expression for the arc in the positive series contains a function of the third kind, the parameter of which is alternately circular and logarithmic; the curves of an odd order involving the same function of the circular kind, and those of an even order the same of the logarithmic kind, if the real axis of the base-hyperbola be greater than the imaginary, and *vice versâ*.

Mr. Roberts also shows, that besides the case of the equilateral hyperbola, in which the first positive curve is the lemniscate of Bernoulli, and which has been the only one hitherto noticed, at least as far as he is aware, there are two others, in which the arc of the first positive curve can be expressed by a function of the first kind, with the addition of a circular arc in one case, and of a logarithm in the other. The first of these occurs when the imaginary semiaxis is equal to $\frac{\sqrt{5}-1}{2}$ (the distance between the centre and focus being unity), and this fraction is the modulus of the function. The other case is furnished by the conjugate hyperbola, and the modulus is complementary. In both these cases functions of the third kind disappear from the arcs of the positive curves.

If the hyperbola be equilateral, and its semiaxis be supposed equal to unity, the general equation of the derived curves of both series may be presented under the form

$$r_{\pm n}^{\frac{2}{\pm 2n-1}}=\cos\left(\frac{2\,\omega_{\pm n}}{\pm 2\,n-1}\right).$$

The successive curves represented by this equation are very curiously related to each other. The following property appears worthy of remark:—

Let P_{n-1}, P_n, P_{n+1} be corresponding points on the $(n-1)$th, nth, and $(n+1)$th curves of the positive series respectively, and V their common vertex, which is also that of the hyperbola, then will

$$\text{arc } V P_{n-1} + \text{right line } P_{n-1} P_n = \frac{2n-1}{2n+1} \text{ arc } V P_{n+1}.$$

Mr. Roberts states that he has demonstrated the property in a manner purely geometrical.

This equation shows that the arcs of all the curves of an odd order will depend only on that of Bernoulli's lemniscate, or the function F $\{ \sqrt{\tfrac{1}{2}}, \phi \}$, and those of an even order only on the arc of the second of the series. This latter arc is three times the difference between the corresponding hyperbolic arc and the portion of the tangent applied at its extremity, which is intercepted between the point of contact and the perpendicular dropped upon it from the centre: and the entire quadrant is three times the difference between the infinite hyperbolic arc and its asymptot.

Also, S_n, S_{n+1}, denoting the quadrants of the nth, and $(n+1)$th curves, the following very remarkable relation exists between them:

$$S_n\, S_{n+1} = (2\,n+1)\,\frac{\pi}{4}.$$

The curves of the negative series enjoy analogous properties.

Lastly, let the base curve be a circle, the origin being within it: and it appears that the rectification of the curves of both series, which are of an even order, can be effected by the arcs of circles; and that those of an odd order, which belong to the positive series, will involve elliptic integrals of the first and second kinds in their arcs. The negative curves of an odd order contain a term depending on a function of the third kind, which is however reducible to a function of the first kind and a logarithm.

By the particular consideration of the first negative curve in this case, Mr. Roberts was led to a very simple demonstration of the equation which results from the application of Lagrange's celebrated scale of reduction to elliptic functions of the second kind, and which is nothing more than the analytical expression of Landen's theorem.

William Roberts, Esq., F.T.C.D., read a paper on a class of spherical curves, the arcs of which represent the three species of elliptic transcendents.

A cone of the second order, whose vertex is upon the surface of a sphere, and one of whose principal axes is a diameter, will intersect the sphere along a curve which admits of several varieties, according to the nature of the sections of the cone parallel to its principal planes, and the position of its internal axis. This curve may be made to furnish, by means of its arc, a geometrical representation of the three species of elliptic trancendents, including the two cases of the third.

In the course of the investigations alluded to, Mr. Roberts was also led to consider two species of the curve called the spherical conic, which appear to possess many remarkable analogies to the

properties of the equilateral hyperbola. These cases occur when the axis minor is a quadrant, and when the semi-axes a and b are connected by the relation

$$\sin a = \tan b.$$

A notice of the occurrence of a Metallic Alloy in an unusual state of aggregation and molecular arrangement, was read by Robert Mallet, Esq., M.R.I.A,

Amongst the several classes of substances which chemistry at present considers as simple, the metals stand preeminently marked by their almost invariable possession of a nearly fixed and striking group of sensible qualities, which together constitute the well-known "metallic character." Some of these, such as lustre and fusibility, are common to every metallic body; but by the occasional variation of nearly every other sensible quality of the metals, the law of continuity remains unbroken, which unites them in different directions with the other classes of material bodies. Thus opacity, which is probably mechanically destroyed in gold leaf, is lost in selenium; and so, in this most prevalent of their properties, the metals, through tellurium, selenium and sulphur, become translucent, and mingle with the non-metallic elements. So also their solidity, at common temperature, is lost in mercury; their great density, in sodium and potassium; their malleability, in bismuth, antimony, and arsenic; while in tellurium, the power to conduct electricity is nearly wanting; and, lastly, hydrogen, to all intents a metal in its chemical relations, yet possesses not a single physical quality in common with these, but exists as an invisible and scarcely ponderable gas.

But although *different* metals thus vary in sensible qualities, those which collectively belong to *the same* individual metal are as remarkable for their permanence.

Unless selenium be admitted to be a metal, no approach to dimorphism has hitherto been recognized in any body of the class; the only case recorded, that by Dufresnoy, of the occurrence of cast iron in cubes and rhomboids, not having been given by him with certainty, nor since verified by other observers. Hence any instance of such a character, or tendency towards it, is worthy of attentive consideration; and it was with this view that the author brought before the Academy the following notice of the occurrence of an alloy of copper, in two states, having totally different sensible and physical qualities, while identical in chemical constitution. The alloy in question, in its original or normal condition, was in fact a species of brass; and the particular specimen presented to the Academy was a portion of one of the brass bearings, or beds, in which the principal shaft of a large steam-engine revolved.

The bearing, or bed of a shaft (as is generally known), consists of a hollow cylinder, generally of brass, divided in two by a plane passing through the axis; its inner surface is finely polished, and sustains the shaft, during its revolution, which is also polished; the cavity of the brass being completely filled by the shaft, which, in the present instance, was of cast iron, and about nine inches in diameter.

It frequently happens, notwithstanding the polish of both metallic surfaces, and the application of oil, that the friction due to their rapid passage over each other, while exposed to undue or irregular pressure, produces a considerable rise of temperature, and the brass becomes abraded. Its particles have no coherence, and much resemble the "bronze powder" used by painters.

In an instance, however, which some time since came under the author's notice, a different result took place. The minute particles of abraded brass were by the motion of the shaft, during a few hours, impacted into a cavity, at the junction of the two semicylinders of the bearing, where they became again a coherent mass, and when removed presented all the external appearance of an ingot or piece of brass which had been poured in a state of fusion into the cavity. On more minute examination, however, the mass was found to differ much in properties from the original brass, out of which it was formed.

The mass or ingot of brass, thus formed by the union of particles at a temperature which had never reached that of boiling water, and a fragment of which was presented, possessed on that side which had been in contact with the shaft, a bright polished metallic surface, like that of the original metal from which it had been formed: its other surfaces bore the impress of the cavity in which it was found. It was hard, coherent, and could be filed or polished like ordinary brass. It was, however, perfectly *brittle*; and when broken, the fracture, in place of possessing a sub-crystalline structure and metallic lustre, like that of the normal brass or alloy, was nearly *black*, and of a fine grained *earthy* character, and *without any* trace of *metallic lustre* or appearance.

Examined with a lens, some very minute pores or cavities are found throughout its substance, which is uniformly of a very dark brown or nearly black colour, and devoid of all metallic character, except when cut or filed—that is, in minerological language, its colour is earthy black, and its streak metallic.

The author remarked that the observed cases of aggregation in solid particles, without the intervention either of a solvent or of fusion, are extremely rare, and as bearing upon the little understood subject of cohesive attraction, are of much interest.

The property of welding, which is possessed by all bodies, whether metallic or not, which pass through an intermediate stage of softness or pastyness previous to fusion, and is not found in any substance which readily crystallizes, and hence passes *per saltum* from the solid to the liquid state by heat, forms a "frontier instance" of cohesive forces, being enabled to act in the aggregation of bodies, by only an approach to liquidity, or by a very small degree of intermobility.

Aggregation may also take place between portions of a body merely *softened* by a solvent, which is afterwards withdrawn, as in the familiar instance of Indian rubber, softened by naphtha for the manufacture of waterproof cloths; where the former, after being moulded or united in any way required, is left in its pristine condi-

tion hy the evaporation of the naphtha from amongst its particles. But the cases of aggregation of solids, without such elevation of temperature, or the presence of solvents, are so rare, that but two or three have as yet been observed. Of these the most remarkable is that recorded by Pouillet, of the gradual, hut complete, adhesion of surfaces of clean plate-glass, when left to repose on each other for a considerable time. It has also been stated, that clean plates of lead or of tin, if pressed together by a considerable force when cold, require a proportionably great force to separate them. The case presented to the Academy, therefore, is another added to these rare instances of molecular aggregation in solids, independent of solution of fusion: the author therefore thought it worth while to examine with a little care the properties both of the original hrass, and of the mass thus curiously formed from it, or, as he thenceforth called them, of *the normal* and *the anomalous* alloy.

The normal alloy is of a hright gold colour, and sub-crystalline in structure, and of great toughness; its cohesive force is equal to 21·8 tons per square inch, which is above the averagc strength of any of the alloys of copper and zinc, or copper and tin, as found hy my experiments on the cohesive power of these alloys, puhlished in the Proceedings of the Academy, and elsewhere. The cohesive force of the anomalous alloy is only 1·43 ton per square inch, or only about one-fifteenth that of the former.

The specific gravity of the normal alloy is = 8·600; that of the anomalous only = 7·581.

On submitting both alloys to analysis, their constitution proved identical; it is as follows:—

Copper	83·523
Tin	8·833
Zinc	7·510
Lead	0·024
Loss	0·110
	100·000

Uniting the small amount of lead with the tin, and dividing hy the atomic weights, the nearest approach to atomic constitution is,—

Copper	=	26·3 atoms.
Zinc	=	2·3 ...
Tin	=	1·5 ...

These alloys have therefore not a strictly definite constitution, but one more nearly so than is usually found in commerce.

Both alloys are equally good conductors of electricity. The author examined their relative powers of conducting heat by the method which Despretz has employed with so much accuracy, and found that of the normal to that of the anomalous alloy as 36 : 35, numbers which are so nearly equal as to render it likely the difference is only error of experiment. He also endeavoured to determine their relative specific heats, using the method of mixture, which was the only one which the small size of the metals permitted, and eliminating the errors incident to this mode by first plunging the

alloy hot into cold water, and then cold into hot water. In this way, if

W and t = the weight and temperature of the water,
M and t' = the weight and temperature of the metallio alloy,
m .... = the mean temperature of both,
S .... = the specific heat of the alloy,

there are two values, one where the metal is the hotter,

$$S = \frac{W(m-t)}{M(t'-m)};$$

and another where the water is the hotter body,

$$S = \frac{W(t-m)}{M(m-t')};$$

the mean of which is the specific heat of the alloy pretty exactly. The result gave the specific heat of the normal alloy = ·0879, water as unity, and that of the anomalous alloy = ·0848; both of which are below the specific heat assigned by Dalton to brass.

The normal alloy is malleable, flexible, ductile, and laminable. In the anomalous alloy there is an absolute negation of all these properties.

The normal alloy readily amalgamates with mercury at common temperatures; the anomalous alloy will not amalgamate with mercury even at 400° Fahrenheit.

When the anomalous alloy is heated to incipient redness in a glass tube, a minute trace of water, and of a burned organic substance, probably adherent oil, are discoverable; it suffers no change, however, but a slight increase of density. The normal alloy suffers no change when so treated. The normal alloy, treated on charcoal with the blowpipe, fuses at once into a bead. On treating the anomalous alloy so, the fragment swells rapidly to more than twice its original bulk, on becoming bright red-hot; it then glows, or becomes spontaneously incandescent, in the way that hydrated oxide of chrome and some others do, and instantly contracts to less than its original bulk, and becomes a fluid bead, which, on cooling, differs in no respect from the original alloy.

The anomalous alloy, when pulverized in an agate mortar, forms a *black powder*, devoid of all appearance of a metal; its filings also are quite *black*; while those of the normal alloy, produced by the same file, possess the usual metallic lustre. These facts, in connexion with the black colour and fine earthy appearance of the fracture, bring to mind the case recorded by Sir David Brewster, of a piece of smoky quartz, the fracture of which was absolutely black, and yet was quite transparent to transmitted light, and whose blackness, he found, arose from the surfaces of fracture, consisting of a fine down of short and slender filaments of transparent and colourless quartz, the diameter of which was so small (not exceeding the one-third of the millionth part of an inch), that they were incapable of reflecting a single ray of the strongest light. In describing this, Sir David Brewster predicted that "fractures of quartz and other minerals would yet be found which should exhibit a fine down of different colours depending on their size."

It seems, therefore, extremely probable, that the cause of the near approach to blackness in the fracture and filings of this alloy, arises from the excessive minuteness of its particles, and thus fulfils the foregoing prediction; the brownish tinge being produced by the reflexion of a little red light *.

The polish and power of reflecting light of the anomalous alloy are not quite so great as those of the normal, but are still remarkable; and, as it seemed a matter of some interest to determine whether both reflected the same quantity or intensity of light at equal angles, the author endeavoured to ascertain this point as respects heat, by means of Melloni's pile for the galvanometrical determination of temperature, assuming, as suggested to him by Professor MacCullagh, that what would be true of heat in this respect, would also be so of light; but from the small size of the reflecting surfaces he had at his command, he found it impossible to arrive at any trustworthy result. He is, however, inclined to believe, that both metals reflect most at a perpendicular incidence.

From the foregoing detail of the properties, in several respects so different, of this substance in its normal and anomalous states, the author thinks he is warranted in pronouncing it the first observed instance of an approach to dimorphism in a metallic alloy; and one, the mode of production and characteristics of which present several points of interest.

The conditions under which the alloy was aggregated, involved extremely minute division of the metal, great pressure in forcing the divided particles into contact, and nearly the exclusion of air. Considerable electrical disturbance may have also cooperated; such, together with induced magnetism, being the constant accompaniments of motion in heavy machinery. By re-establishing these conditions, under suitable arrangements, the author hopes to repeat the results thus accidentally first obtained, and so produce possibly dimorphous states of other metals or their *definite* combinations.

There is but one body which occurred to the author presenting an analogy to this anomalous alloy, namely, indigo; whose fracture, it is well known, is fine earthy, and of the usual blue colour, but becomes coppery, or assumes the metallic lustre on being rubbed or burnished.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxii. p. 570.]

June 9, 1843†.—The following communications were read:—

I. On a Self-acting Circular Dividing Engine. By W. Simms, Esq.

The original graduation of a circle, notwithstanding the great improvements in the method invented by Mr. Troughton, is still attended with very great difficulties, requiring not only the greatest

* Since this paper was read, Professor Lloyd suggested to the author, the analogy between the appearance of the powder and filings of the anomalous alloy and Platina Mohr, and those powders obtained by reduction of other metals by hydrogen. None of these, however, are coherent, which constitutes the peculiarity in the present case.

† The proceedings of the Society in April and May will be noticed hereafter.

care on the part of the operator, but tending to injure his health by the labours required in it, and thus not admitting of frequent repetition. The necessary cost of an instrument produced by such an amount of severe labour is also another very serious objection. The author had long been of opinion, that to copy the divisions of a circle which had been graduated with extraordinary care, upon work of smaller dimensions, would in general be more satisfactory than original graduation. The latter process consists of several successive steps, in either or all of which a certain amount of error may escape detection, which in general may go far to balance one another, although there will be parts in almost every work where errors appear arising from an accumulation of those minute quantities.

The author had long since determined, as soon as he could obtain sufficient leisure, to construct an engine sufficiently large for the graduation of all circles, excepting those of the largest class, and the object of this paper is to lay before the Society a brief notice of the successful termination of the work.

The engine, in general arrangement and construction, is similar to that made by Mr. Edward Troughton, in the author's possession, though there are several additions and peculiarities which are pointed out by him. The circle or engine-plate is of gun-metal, 46 inches in diameter, and was cast in one entire piece by Messrs. Maudslay and Field, teeth being ratched upon its edge. The centre of the engine-plate is so arranged that it can be entered by the axis of the instrument to be divided, and the work by this means brought down to bear upon the surface of the engine-plate, which arrangement prevents the necessity of separating the part intended to receive the divisions from its axis, &c.—a process both troublesome and dangerous.

Upon the surface, and not far from the edge of the engine-plate, are two sets of divisions to spaces of five minutes, one set being in silver and the other strongly cut upon the gun-metal face. There are also as many teeth upon the edge as there are divisions upon the face of the engine-plate, namely, 4320, and consequently one revolution of the endless screw moves through a space of five minutes. The silver ring was divided according to Troughton's method with some slight variations. In this operation it seemed to the author the safer course to divide the circle completely, and then to use a single cutter for ratching the edge; and he believes that the teeth upon the edge have been cut as truly as the original divisions themselves.

Another very important arrangement is, that the engine is self-acting and requires no personal exertion or superintendence, nothing being necessary but the winding up of the machine, or rather the raising of a weight, which, by its descent, communicates motion to the dividing engine. The machinery is so arranged that it can be used or dispensed with at pleasure, there being some cases in which a superintending hand is desirable.

The author then proceeds with a description of the machinery, as represented in the drawings accompanying his paper, and draws particular attention to the contrivance by which the engine can discharge itself from action when it has completed its work.

He concludes by observing, that as the machinery is simple, by no means expensive, can be made by an ordinary workman, is adapted to all the engines now in existence which are moved by an endless screw, as it lessens the labour of the artist and increases the accuracy of the graduated instrument, he trusts his communication will prove acceptable to all who are interested about such matters.

II. Recomputation of Roy's Triangulation for connecting the Observatories of Greenwich and Paris. By W. Galbraith, Esq.

The author considers from internal evidence that Roy's measurement of the base on Hounslow Heath was in his own scale, and that of Mudge in Ramsden's scale, and he has used in his calculations the mean of these in imperial measure, reduced to the mean level of the sea. He has also availed himself of the New Survey of France to obtain such data as it afforded to connect the two countries.

Some of the most important of the results are as follows:—Assuming the latitude of Greenwich to be 51° 28′ 38″·50 N., and the compression of the earth to be $\frac{1}{300}$, there results for

Calais	Lat. 50° 57′ 27″·67 N.;	Long. 1° 51′ 17″·30 E.
Dunkirk	Lat. 51 2 6·68 N.;	Long. 2 22 39·72 E.

Again, assuming from the new *Déscription Géometrique de la France*, the long. of Calais to be 29′ 0″·40 West of Paris, and that of Dunkirk 2′ 22·″66 East of Paris,

	h	m	s
The long. of Paris by comparison with Dunkirk is	2	20	17·70 E.
... Calais	2	20	17·06
... Calais, from Kater's New Survey	2	20	19·13

	h	m	s
The Mean of which in Time is	0	9	21·20 E,
Mr. Dent's Result by Chronometers was	0	9	21·21
And Sir J. Herschel's by five signals	0	9	21·46

III. Occultations of Fixed Stars and the Planet Jupiter by the Moon. Observed at Hamburgh by C. Rumker, Esq. These will be found in the Monthly Notices of the Society, vol. v. p. 293.

IV. The following communications concerning the Great Comet of 1843*:—

1. Notes on its Appearance made during a Voyage from the Cape of Good Hope to England. By M. Close, Esq., Commander of the Ship Ellenborough.

It was first seen on the evening of the 4th of March, and before the discovery of the nucleus on the same evening was taken for a lunar iris. The nucleus was on this evening estimated to be of equal brightness with a star of the second or third magnitude, and the length of its tail 32° 30′. The tail had a darkish line from its nucleus through the centre to the end. Stars of the third magnitude were visible through the broadest part, but not near the nucleus. It was seen on several evenings, the last time being on the evening of the 31st of March; it was occasionally brilliant enough to throw a strong light on the sea. The greatest length of tail estimated was on the 19th of March, it being then 43° 30′, and it was observed to

* See also p. 54 of the present, and p. 323 of the preceding vol.—Edit.

have considerable curvature. The account was accompanied by a small sketch of its appearance on the 5th of March.

2. A Letter from John Belam, Esq., Master of H. M. Sloop Albatross, on the Great Comet of 1843. Communicated by G. B. Airy, Esq., Astronomer Royal.

"On the 2nd of March, in latitude 18° 33′ 18″ N. and longitude 72° 17′ 0″ West of Greenwich, a meteor made its appearance in the western quarter of the heavens of a whitish colour. It became brighter each succeeding evening, and on the 7th we obtained the following observations:—In form it is like an elongated birch rod, slightly curved: the head or commencement of it being nearest the horizon at an altitude of about 19°, the tail pointing in the direction of Sirius, and measuring from the head 28°. That part of it from which the tail is produced is of a reddish appearance, but no star is visible through a common telescope.

	Greenwich Mean Time.	Observed Distance between	
	h m s		° ′ ″
March 7.	11 54 56	The Comet and Sirius	83 12 10
	11 56 33	... α Canopus..	75 25 40

Observed Azimuth South, 79° 10′.

	Greenwich Mean Time.	Observed Distance between	
	h m s		° ′ ″
March 13.	11 52 46	The Comet and α Canopus	66 55 10
	11 54 46	... Sirius ...	67 1 40
	11 55 46	... Aldebaran	44 26 30

Its altitude appears to be about 23°; its azimuth (observed), South 79° 11′.

"It showed brightest on the 7th instant, and since then to the 13th it has gradually become fainter.

"Since its appearance the generative point has been surrounded by a misty haze, from which cause the observations could not be taken with any great precision.

"H. M. Sloop Albatross, Port au Prince,
St. Domingo, 14th March, 1843."

3. Observations of the Comet by Mr. S. C. Walker and Professor Kendall, at the Observatory of the High School at Philadelphia. Communicated by Lieut.-Col. Sabine.

The comet was observed with the 9-feet Fraunhofer equatoreal, and the observations are cleared of the effects of differences of refraction, but not of the effects of parallax and aberration. Latitude of the Observatory, 39° 57′ 8″; longitude, $5^h\ 0^m\ 41^s\cdot9$.

Date of Observation. Sidereal Time.	Apparent Right Ascension.	Sidereal Time.	Apparent Declination.
d h m s	h m s	h m s	° ′ ″
March 19 7 32 6·7	2 57 24·06	7 32 6·7	−9 27 36·5
22 7 48 45·3	3 17 43·34	7 44 49·2	−8 36 3·4
23 7 40 33·7	3 23 43·97	7 40 50·1	−8 19 20·8
24 7 35 52·5	3 29 36·28	7 46 32·9	−8 3 33·0
26 7 57 24·3	3 40 30·54	8 6 54·5	−7 32 32·2
29 8 11 35·1	3 54 14·26	8 14 10·0	−6 50 3·1

The observations of the 19th, 22nd, and 24th, by computations made by the same gentlemen, give the following approximate elements :—

Perihelion Passage, Feb. 26·0489, mean time Philadelphia.

Ascending Node	166° 1′ 25″
Inclination	39 0 22
Longitude of Perihelion	292 50 31
Perihelion Distance........	0·00834

Motion direct.

4. Notes on the Comet, accompanied by a Pencil Sketch, by Capt. Hopkins, commanding the East India Company's Ship Seringapatam, on a voyage from the Cape of Good Hope. Communicated by Sir John Herschel.

The comet was seen first on the 2nd of March, but indistinctly. A good view of it was obtained on March 4, when it was very brilliant. Its tail appeared separated through half its length by a dark line, and was, by rough measurement, found to be about 30° in length. After this it decreased in brightness, and was not seen longer than the 7th of April.

5. Extract of a Letter, dated St. Kitt's, 6th of March, 1843, from Lieut. D. W. Tyler, R.E.

6. Letter from J. T. Austin, Esq., dated Funchal, Madeira, April 8, 1843, accompanying a sketch of the Comet. Communicated by Sir John Herschel.

7. Notes on the Comet as seen by M. Montojo, at San Fernando. Communicated by Sir John Herschel.

The tail of the comet was first seen on the 6th of March; the nucleus was compared roughly with two small stars seen in the same field with it on the 13th; on the 14th and 15th some observations were obtained with an altitude and azimuth instrument, and it was compared with some known stars; the nucleus was not seen after the 1st of April.

8. An Account of the Comet as seen on board the ship Childe Harold on her voyage from Bombay to London. By Lieut. W. S. Jacob, R.E.

The tail was first seen on the 3rd of March, but a good view of it was not obtained till the 9th. On this evening, the nucleus seen in a night telescope appeared like a star of the sixth magnitude, and the following distances from α Eridani and α Orionis were measured with a sextant :—

Time by watch h m	° ′	
8 15	46 11	Distance from α Eridani.
16	12	
25	8	
26	10	
18	56 35	Distance from α Orionis.
19	34	
21	34	
23	34	

Watch fast on Greenwich mean time (by estimated long.) $39^s \cdot 0$.

The length of the tail 36°.

From the above observations Mr. Jacob infers the following place of the comet :—

At $7^h\ 42^m$ Gr. M.T. R.A. $= 1^h\ 17^m\ 9^s$ Decl. $= -\ 11° 59'$.

The nucleus was last seen on the 5th of April.

9. Letter from T. Forster, Esq., dated Bruges, April 22, 1843.

Dr. Forster, with a view of drawing attention to the phænomena observed by him on the 20th of March in connexion with the comet, had accurately represented in a coloured drawing the appearance of the comet and of the surrounding sky, and had caused it to be copied by an artist, with the intention of presenting the same to the Society. The drawing has since been received, and is now in the possession of the Society.

10. An Account of the Comet as seen on board the ship Malabar on her passage from the Cape of Good Hope. By R. Pollock, Esq., Commander.

The comet was first seen on the 2nd of March. On the 5th the nucleus was well seen, and appeared as a star of the fourth magnitude; the length of tail was 23°.

The following measures of the distance of the nucleus and bright stars were made :—

	h m			° ′ ″	
March 10.	7 0	Dist. from	Regulus	53 20 0	Long. 7° 22′ W.
	7 35		Sirius	74 33 30	
	7 40		Canopus	70 6 0	
11.	7 35		Sirius..	71 41 0	Long. 9 50 W.
	7 40		Canopus	68 47 0	
13.	7 35		Sirius	67 0 0	
	7 40		Canopus	69 30 0	
14.	No observations..................				Long. 15 55 W.

11. Letter from H. A. Cowper, Esq., H. M. Consul at Pernambuco in Brasil, dated 9th March, 1843.

The comet was seen first on March 1; and on the 4th Mr. Cowper saw the nucleus very distinctly, and makes the following remarks on its appearance :—

"It is particularly small, without any nebulosity, but of extreme brightness, of a golden hue, and a line of the same bright colour may be distinctly traced running directly from it into the tail for 4° or 5°: the tail is perhaps 30° in length, and is of a brilliant silver colour, perfectly opake, but becoming less and less dense until it is lost in space."

Mr. Cowper adds the following observations, made with a sextant on March 9, at his request, by a master of a merchant vessel:

		° ′	
Bearing of nucleus.. ...	W.	7 45	S.
Altitude of ditto		9 0	
Length of tail		28 0	

Breadth of tail at two-thirds of its length from the nucleus 1°.

12. Observations made at the Royal Observatory, Cape of Good

Hope, by Piazzi Smyth, Esq. Communicated by Sir John F. W. Herschel, Bart.

The nucleus of the comet was first seen on the 3rd of March, but it set about ten minutes after its discovery. It was looked at with the 46-inch achromatic telescope, and an approximate observation was attempted, by leaving the telescope fixed, and measuring, the next morning, the azimuth and altitude of the point where it set.

The nucleus seemed to consist of a planetary disc from which rays emerged in the direction of the tail. To the naked eye there appeared a double tail about 25° in length, the two streamers making with each other an angle of about 15′, and proceeding from the head in perfectly straight lines. From the end of the forked tail, and on the north side of it, a streamer diverged at an angle of 6° or 7° towards the north, and reached a distance of upwards of 65° from the comet's head; a star (probably τ Ceti) was near the end of this appendage; a similar, though much fainter, streamer was thought to turn off south of the line of direction of the tail.

On the 4th of March, Mr. Smyth, accompanied by some friends, went to the Lion's Rump signal station, where the comet would set in a sea horizon, and several distances were taken with sextants and a reflecting circle. These not being reduced sufficiently are not inserted here.

On the 5th the comet was seen, and several sextant observations were made. The appearance of the comet on this evening appeared considerably changed; the angle of the north streamer with the direction of the tail had been diminishing and was now south; it had also diminished in brightness. The total length was about 35°. All the rays proceeding from the head were now of uniform brightness, excepting one bright streak, which could be traced along the tail.

Though the observatory is very deficient in extra-meridional apparatus, Mr. Smyth succeeded, on March 6, by various expedients, in obtaining several comparative measures of the nucleus and neighbouring stars, of which the unreduced observations only are given. On this evening he makes the following remarks respecting the appearance of the comet:—"The nucleus is now the broadest part of that end of the comet; all the rays come from the posterior side, and are pretty equal in brightness, with the exception of a narrow bright streak in the middle, which runs for 3° or 4° along the middle of the tail, and then verges to the north side." The tail this evening was about 27° long. Several sextant observations of distance were made this day.

On the 8th several differential observations were made.

On the 9th some good differential observations and some sextant observations were made. The angle of the two sides of the tail at the head appeared to have undergone a gradual diminution, and the middle part was becoming more and more equal in brightness to the sides.

The paper contains also some observations of Laugier's Comet, and some observations of occultations of stars by the moon.

13. Abstract, by the Secretary, of Newspaper Accounts of the Comet which have been forwarded to the Society.

Some of these accounts are of considerable interest, and Mr. Main thought it desirable to collect and to bring them before the Society, which will thus be in possession of almost every thing that has been published relatively to the comet.

The first is extracted from a newspaper of Tobago, and is signed M. Dill, Lieutenant Royal Engineers, Superintendent of Signals, and dated Fort King George, 8th March, 1843.

"During the last few days our island has been visited by a very large and brilliant comet. It is said to have been seen first on Friday evening, but was more generally observed on Saturday, when it presented a most luminous appearance; the nucleus of the comet being, at about ¼ to 7 o'clock, in the direction of south-west, and the tail stretching to an immense length across the heavens in a south-east direction, and in opposition to the sun's light. On Sunday night it was observed by compass, when the nucleus bore west 25° 0′ south at ¼ to 7 o'clock. On Monday an observation was taken with a theodolite at $16^{m}\frac{1}{2}$ past 7 o'clock, when the nucleus bore west 16° 7′ south, with an elevation of 6° 4′, and the end of the tail, as nearly as it could be caught, bore west 28° 14′ south, with an elevation of 28° 21′."

Similar observations were made of it on the following Tuesday and Wednesday.

The following letter from Mr. Benjamin Pierce, dated Cambridge, March 31, 1843, appeared in the Boston Courier of April 1, 1843:—

"The elements of the comet's orbit, which I send you, are roughly computed, and will need future correction. They agree very closely with Mr. Clarke's noonday observations of February 28, and were computed from Mr. Bond's observations of March 11, 18, 24, and 26. More correct calculations, in which all the observations will be thoroughly discussed, will in due time be presented to the American Academy.

Long. of the Ascending Node	348° 33′
Inclination......................	39 16
Long. of the Perihelion............	280 31
Perihelion Distance	0·00872

Time of Perihelion Passage, Feb. $27^{d}\cdot01$, mean time at Cambridge. Motion retrograde."

In another American paper was the following account:—

"A comet of unusual size and brilliancy was distinctly visible to the naked eye in this vicinity on Tuesday last, the 28th of February, 1843, at noonday, at a distance, as we should judge, of 5° or 6° east from the sun. It extended over a space in the heavens of nearly 3° in length, with little more than 1° in width, and appeared like a very small white cloud, with its nucleus, or densest part, towards the sun, and its luminous train in opposition to it. On viewing it through a common telescope of moderate power, it presented a distinct and beautiful appearance, exhibiting a very white and bright nucleus,

and a tail dividing near the nucleus into two separate branches, with the outer sides of each branch convex, and of nearly equal length, apparently 8° or 10°, and a space between their extremities of 5° or 6°. Though viewed several minutes under these favourable circumstances, no coruscations were perceived."

The above American accounts were communicated to the Astronomer Royal by Mr. J. Cranch, at the request of Mr. Bond, of the Cambridge Observatory, near Boston.

In an article by M. Plana, extracted from the *Gazetta Piemontese* of the 4th of April, are the following parabolic elements of the comet :—

Perihelion Passage, 1843, Feb. 27·652, Munich mean time.

Perihelion Distance	0·0056343
Long. of the Perihelion..........	189° 51′ 25″
Inclination....................	40 29 37
Long. of the Ascending Node	353 0 59

Motion retrograde.

The following observations and elements of the comet, given by M. Carlini, Director of the Royal Observatory of Milan, are extracted from an Italian Gazette :—

1843.	Mean Time.	Right Ascension.	Declination.	Longitude.	Latitude.
	h m	° ′	° ′	s ° ′	° ′
March 19.	7 37	43 33	−9 31	1 8 18	−25 5
29.	8 14	58 30	−6 52	1 24 34	−26 32
30.	8 14	59 38	−6 38	1 25 51	−26 33

The above observations, together with one made at Munich on the 23rd of March, and another made at Padua, on the 24th, furnished the following elements :—

Perihelion Passage, Feb. 27, 5^h mean time of Milan.

Perihelion Distance	0·1542
Long. of the Perihelion	243° 33′
Long. of the Node	353 45
Inclination	38 0

Motion retrograde.

In the Guiana Herald (city of Georgetown) of March 30, 1843, appeared a notice of the comet, dated Demerara, March 25, and signed J. Bamber, with observations of its distance from neighbouring bright stars; the most important of which are as follow :—

" Saturday, March 18*, at 7^h 14^m, the nucleus was brilliant; the coma, body and tail, very transparent.

h m		° ′ ″
At 7 23	Sirius and the Nucleus, Apparent Distance	55 56 0
7 28	γ Orionis	40 16 7
7 45	Aldebaran	35 11 0

" Saturday, March 19. Appearance as before. The evening was beautiful.

* Some of the dates in these Demerara observations are evidently erroneous.—*Edit. Phil. Mag.*

h m		° ′ ″
At 7 7	α Orionis and Nucleus, Apparent Distance	47 0 0
7 11	γ Orionis	38 14 0
7 14	Rigel	32 32 9
7 17	Aldebaran	32 2 0
7 22	Sirius	55 9 0

" Sunday, March 29.

h m		° ′ ″
At 7 5	Rigel and Nucleus, Apparent Distance ..	26 56 30
7 11	α Orionis	38 4 0
7 22	Sirius	45 13 0
7 33	Aldebaran	26 32 0
7 57	Sirius	44 25 0

" Monday 27. The nucleus very faint, as also the body and tail.

h m		° ′ ″
At 7 33	Rigel and Nucleus, Apparent Distance...	21 0 0
8 3	Procyon	52 50 0

" It appears that the comet was first seen in this colony on March 3."

The newspaper containing the above account was received by the Astronomer Royal, and communicated to the Society by him*.

XX. *Intelligence and Miscellaneous Articles.*

ON THE VARIATION OF GRAVITY IN SHIPS' CARGOES IN DIFFERENT LATITUDES.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your valuable publication for April last (S. 3. vol. xxii. p. 326), you kindly inserted an article of mine entitled " On the Effect of the Variation of *Gravity* on Ships' Cargoes in different Latitudes." On turning to my manuscript, I find the article there headed, " On the Variation in the *Weight* of Ships' Cargoes in different Latitudes." As *weight* and *gravity* are not exactly synonymous, perhaps this title is less ambiguous than that which was printed; but be that as it may, a very cursory perusal of the article is quite sufficient to show that its design was to exhibit the variation in *weight* of cargoes in different places through which a ship may be supposed to pass. The table adverted to in your note showed the weight of the burden of different vessels in each degree of latitude, they having a given tonnage in the latitude of London. Whatever may be the merits or demerits of the little article, I wish there to be no mistake; the article states the circumstance that suggested it. The table was constructed purposely to show how the *weight* of a body varies, supposing it to be carried through different latitudes; without the table the communication was rather incomplete, but as no room could be found to squeeze it in, it is useless to say more about it.

Your Correspondent K. K., in the present (June) Number (vol. xxii. p. 503), in adverting to my communication, says, " the author

* These communications relative to the Comet will be continued in our next.

appears to have forgotten that *two* scales are commonly used in weighing goods, and that the weights in *both* must be equally affected by the 'variation of gravity.'"

I suppose I may take it for granted that your Correspondent read the article as well as its title; if he did, I think he must have perceived its design, and if so, may not I ask, "Suppose I did forget the two scales in forming that communication, what then?" What have *two scales* to do with the matter? Besides, *two scales* are not absolutely necessary. Perhaps K. K. has seen large parcels weighed in coach-offices, &c. by being suspended to a piece of mechanism having a moveable indicator that points out the weight on a dial-plate. Now were this apparatus, and a heavy weight suspended to it, carried from London to Madras, does K. K. maintain that the indicator would point to the same figure at each place? If I read his comment correctly it implies that such would be the case. I affirm that it would not, and beg to leave the question for your readers to settle.

Your Correspondent also says, "a ton of any kind of goods weighed at the King's beam in London, and shipped for Madras, will on arriving there counterpoise a standard ton weight as it did before shipment, unless an addition or subtraction of weight has taken place during the voyage."

I suppose this means if a ton of goods be put into one scale in London and a standard ton weight be put into the other scale, they will not only counterpoise each other at London, but in every other latitude; a truism which, no doubt, is quite correct and very sage, and probably was intended to smash my unfortunate communication all to pieces. But your Correspondent has apparently omitted to consider what power supports the beam whilst the bodies are thus counterpoising each other. Some philosophers, it is said, suppose the earth to rest on an elephant, the elephant on a tortoise, and the tortoise on nothing: K. K.'s theory of counterpoising appears to be nearly similar. The *scales* support the goods and standard ton, and the King's beam supports the scales, but the beam hangs upon nothing. If, however, two scales be attached to the King's beam suspended in the usual manner, a ton of goods be put in one scale and a standard ton in the other, perhaps K. K. will admit that the centre of the beam sustains two tons weight at London; but does he mean to say that the centre of the beam would have to sustain the same pressure if the whole were carried to Madras? I deny that it would, and here again beg to refer the point to the decision of your readers. The table that I sent indicated how this pressure varies in different latitudes: if K. K. can prove that the weight of a body is the same in all latitudes, he can show that I am in error, but I must see his proof before I can admit its validity. If two pendulums accurately beat seconds at London, their vibrations will be isochronous; if they are suspended and made to oscillate at Madras, will your Correspondent assert that they will vibrate each once in a second at that place? This question is not irrelevant to the subject under consideration: if K. K. can prove that a body weighs

the same at Madras that it does at London, he can also show that the vibrations of the same pendulum will be performed in the same time at each place.

In forming the little table that I sent to you, I thought there was something novel in its showing how the weight of a body varies in each degree of latitude; at all events it was new to me; but that the weight of bodies does vary when they are carried into different latitudes, was I thought a recognised fact admitting of no dispute. Thus Newton, *Principia*, lib. iii. prop. 20, prob. 4, has "Invenire et inter se comparare *pondera corporum* in terræ hujus regionibus diversis." Having discussed the problem, he says, "Unde tale confit Theorema; quod incrementum ponderis pergendo ab æquatore ad polos, sit quam proximè ut sinus versus latitudinis duplicatæ, vel quod perinde est, ut quadratum sinus recti latitudinis." This problem would seem to have some reference to the subject, but Newton appears to have forgotten the scales.

The 227th question in the Leeds Correspondent is, "To find how much *lighter* the cargo of an East India ship, burden 1000 tons, will be at the equator than when she left the port of London."

Two solutions were inserted; they agree very nearly with the diminution shown in my table. The accomplished editor states that TRUE solutions were also sent by Messrs. Godward and Riley, names quite familiar to readers of mathematical periodicals. The editor however does not remind his correspondents that they have forgotten the scales.

I fear I have occupied too much of your valuable space, but I have been anxious to remove all ambiguity from my meaning. If I am in error, I hope I have shown no wish to conceal it. I am solicitous that your readers, without much trouble, should be enabled to form their own judgement upon the point under discussion: in addition to this, your Correspondent honoured my very trifling communication with an exposition. I trust I have been equally attentive to his production: but, upon the whole, I am sorry that the *title* of the article referred to was not more distinct; and I regret it the more, because after all my endeavours to clear up the matter, I am obliged to conclude my remarks quite in doubt as to whether that somewhat dubious title did not mislead your Correspondent, or whether he had not altogether forgotten to make himself at all acquainted with the subject before he wrote the comment that was published in the last (June) Magazine. This too is a point which I must beg to refer to the judgement of your readers. I remain, Gentlemen,

Your very obedient Servant,

June 15, 1843. J. J.

ON OLIVILE. BY MONS. A. SOBRERO.

Olivile, discovered and analysed in 1816 by M. Pelletier, is very easily obtained by first digesting the powdered resin of the olive-tree in æther, afterwards dissolving the residue in boiling alcohol, and allowing it to crystallize by cooling after filtration. It is easily freed from the resinous matter by which it is rendered impure by washing it on a filter with cold alcohol, which dissolves but very

little of it, and leaves it quite white. By re-dissolving and again crystallizing, it is obtained in small brilliant radiating crystals.

Olivile is readily soluble in alcohol and in water, and crystallizes from either solution; it also dissolves, but in small quantity, in æther, and in the volatile and fixed oils.

Olivile possesses, like lithofellic and silvic acids and some other substances, the property of fusing at different temperatures when crystallized and when amorphous. When crystallized its fusing point is 248° F.; it assumes by this operation a resinous aspect, and neither gains nor loses in weight; on cooling it does not lose its transparency, and splits without re-assuming its crystalline structure; its melting point is then about 158° F. When it is dissolved in alcohol and re-crystallized, its original fusing point of 248° F. is again assumed.

Olivile may be either anhydrous, monohydrated, or bihydrated; the anhydrous is obtained by crystallizing it in anhydrous alcohol, or by fusing the olivile crystallized from water. Its composition is represented by $C^{28} H^{18} O^{10}$.

Olivile crystallized in water, and pressed between folds of filtering paper until it becomes pulverulent and dry to the touch, contains two equivalents of water; it then has the composition indicated by the formula $C^{28} H^{20} O^{12}$.

Olivile by indirect means may be combined with oxide of lead; the salt obtained is represented by an equivalent of anhydrous olivile and two equivalents of oxide of lead.

The composition of olivile, stated by M. Pelletier, does not agree with any of those above stated: he gave its composition atomically as $C^{14} H^9 O^2$, and the composition in 100 parts as—

Carbon	63·84
Hydrogen	8·06
Oxygen	28·10
	100·

No one of the three analyses above described agrees with this statement.—*Journ. de Pharm. et de Chem.*, Avril 1843.

ON SOME NEW COMBINATIONS OF CYANOGEN. BY MONS. A. MEILLET.

The peculiar manner in which cyanogen acts towards iron, by forming two very stable acids with it, leads to the supposition that it is not the only metal with which cyanogen is capable of combining. In fact, some German chemists, and Gmelin among others, have discovered three new compounds, which are platino-cyanogen, cobalto-cyanogen, and chromo-cyanogen, and afterwards the hydrogenated acids analogous to ferro-hydrocyanic acid, and several other metallic salts. The processes employed were somewhat complicated, and they have not continued their experiments. The method which M. Meillet employed is, he says, simple, and there may be procured by it a great number of perfectly definite compounds; the author states, as this inquiry requires much time and care, he shall on the present occasion give merely the principal characters of these salts, reserving their analyses for a future opportunity.

Auro-cyanogen. Auro-cyanide of Potassium.—This salt is obtained by adding a perfectly neutral and saturated solution of chloride of gold to cyanide of potassium. On evaporation and on the cooling of the solution, the salt crystallizes in very white scales, of a pearly lustre; the chloride of potassium and the excess of cyanide of potassium remain in solution. This salt gilds much better than those now employed in the arts.

Platino-cyanogen. Platino-cyanide of Potassium.—Dœbereiner, who discovered this body, prepares it by heating to redness a mixture of equal parts of spongy platina and ferrocyanide of potassium. The residual mass is to be washed, and the undecomposed ferrocyanide is to be obtained first by crystallization, and afterwards the platino-cyanide remaining in the solution, crystallizes when that is concentrated. This process is a long one, and it is difficult to procure a pure salt. M. Meillet prepares it by adding concentrated chloride of platina to a saturated solution of cyanide of potassium; there is immediately formed a precipitate of chloride of platina and potassium mixed with cyanide; it is to be heated to ebullition, and then it redissolves with strong effervescence and disengagement of carbonate of ammonia. It may be supposed that, in this case, the cyanide of platina which is formed reacts like an acid on the atom of cyanate of potash (always contained in this cyanide of potassium), and sets free cyanic acid, which, by absorbing three atoms of water, is converted into bicarbonate of ammonia. Cyanic acid being C^2Az^2O, with three atoms of water H^6O^3, there will be formed an atom of bicarbonate of ammonia, C^2O^4, Az^2H^6.

After the complete solution of the precipitate, the platino-cyanide of potassium crystallizes in blue needles, which are variegated by reflected and yellow by transmitted light.

Cupro-cyanogen. Cuprocyanide of Potassium.—This is prepared by dissolving either cyanide of copper, or carbonate of copper with heat in cyanide of potassium and evaporation; on cooling the salt crystallizes in fine white needles. When, after having poured very concentrated hydrocyanic acid on hydrate of barytes, carbonate of copper is added, it dissolves with strong effervescence, and the liquor assumes a carmine-red tint of extraordinary intensity. On rapidly evaporating the solution, it is gradually decolorated, so perfectly indeed, that on treating the residue with cold water, cuprocyanide of barium is obtained entirely colourless; the cause of this colour was found to be derived from the formation of a considerable quantity of murexide or purpurate of ammonia; M. Meillet endeavoured, but unsuccessfully, to explain the reaction to which its formation was owing, seeing that this body contains a large quantity of hydrogen. Once or twice he found some rudiments of crystals spontaneously formed, which had the colour of cantharides wings, a tint which sufficiently characterizes it. This solution of cuprocyanide of barium, evaporated to dryness, moderately heated, and then treated with water, leaves a residue of carbonate of barytes. On adding a dilute acid, as the hydrochloric, to the cupro-hydrocyanate and purpurate of barytes, purpurate of copper is precipitated, hydrocyanic acid is evolved, and hydrochlorate of barytes only re-

mains in solution. This purpurate [of copper] is a powder of a fine deep violet colour. The red salt of barytes, treated with sulphate of soda, yields sulphate of barytes, and there remains a mixture of purpurate and hydrocyanate of soda, which separate very well by spontaneous evaporation. The purpurate collects on the edges of the capsule; it crystallizes like cauliflowers, and of a fine crimson colour unalterable in the air. The cuprocyanate of sodium remains at the bottom of the capsule in the form of small fine needles, which are also unalterable.

Argento-cyanogen. Argento-cyanide of Potassium.—This salt crystallizes in tables analogous to those of chlorate of potash; it is obtained by dissolving to saturation cyanide of silver in cyanide of potassium; the solution is to be filtered, evaporated, and crystallized.

Argento-hydrocyanic Acid.—This is prepared by dissolving cyanide of silver in cyanide of barium, and precipitating the barytes with sulphuric acid; it is a yellowish acid of considerable stability, possessing the smell of hydrocyanic acid; it is very weak, but still it combines very well with alkaline bases; it acts upon carbonates with greater difficulty.

Hydrargyro-cyanide of Potassium.—This salt is analogous to the preceding, and is prepared in the same manner; it is white, very soluble, and assumes the form of small granular crystals. Similar compounds may be formed with a great number of other salts, as those of cobalt, nickel and cadmium. These the author proposes to describe when he gives the analyses of those already mentioned.—*Journal de Pharm. et de Chimie*, Juin 1843.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1843.

Chiswick.—June 1. Cloudy and fine. 2. Rain: dense clouds: boisterous, with rain at night. 3. Cloudy and fine: clear. 4. Very fine. 5. Fine: heavy showers: clear. 6. Showery. 7. Fine: rain. 8. Cloudy: showery: boisterous, with heavy rain at night. 9. Cloudy and windy: boisterous, with showers and bright sunshine at intervals. 10. Fine: rain. 11. Cloudy and fine. 12. Hazy clouds: rain. 13. Heavy rain. 14. Foggy: cloudy: foggy at night. 15. Hazy: fine. 16—18. Very fine. 19. Overcast. 20. Cloudy. 21, 22. Very fine. 23. Cloudless, with bright sun. 24. Slight haze: fine. 25. Densely overcast. 26. Very fine. 27. Sultry, with hot dry air. 28. Cloudy and fine. 29, 30. Overcast and fine.—Mean temperature of the month about 4° below the average.

Boston.—June 1. Cloudy: rain early A.M. 2, 3. Cloudy: rain P.M. 4. Fine: rain P.M. 5, 6. Fine. 7. Cloudy. 8, 9. Windy: rain early A.M. 10. Windy. 11. Cloudy: rain P.M. 12. Windy. 13. Windy: rain P.M. 14, 15. Fine. 16. Fine: curious halo round the sun 2 to 4 P.M. 17, 18. Cloudy. 19, 20. Windy. 21—23. Fine. 24—28. Cloudy. 29. Windy. 30. Cloudy.

Sandwick Manse, Orkney.—June 1. Bright: damp. 2. Cloudy: drizzle. 3. Rain: showers. 4. Cloudy. 5. Bright: clear. 6. Bright: cloudy. 7. Cloudy: clear. 8. Rain: clear. 9. Damp: drizzle. 10. Showers. 11. Showers: damp. 12. Bright: cloudy. 13. Cloudy: fine. 14. Fine. 15—17. Fine: warm. 18—20. Cloudy. 21. Showers: cloudy. 22. Clear: cloudy. 23. Cloudy: drizzle. 24. Bright: fine. 25. Bright: clear. 26. Bright: cloudy. 27, 28. Cloudy. 29. Drops: showers. 30. Cloudy: damp.

Applegarth Manse, Dumfries-shire.—June 1. Wet all day. 2. Slight showers: warm. 3. Wet nearly all day. 4. Fair and cold. 5. Rain all day. 6. Fair, but cloudy. 7. Rain: thunder. 8. Rain. 9. Showers. 10. Dry and windy. 11—23. Fair and clear. 24. Fair and clear: thunder. 25—28. Fair and clear. 29, 30. Fair and clear: cloudy.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse*, DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse*, ORKNEY.

Days of Month. 1843. June.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¼ a.m.	Barometer. Orkney, Sandwick. 8¾ p.m.	Thermometer. London: R.S. Fahr. 9 a.m.	Thermometer. London: R.S. Self-reg. Mx.	Thermometer. London: R.S. Self-reg. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumf.-shire. Max.	Thermometer. Dumf.-shire. Min.	Thermometer. Orkney, Sandwick. 9¼ a.m.	Thermometer. Orkney, Sandwick. 8½ p.m.
1.	29·716	29·669	29·504	29·04	29·48	29·30	29·74	29·58	61·3	67·6	56·3	67	51	62	55	43	48½	46
2.	29·336	29·305	29·224	28·75	29·18	29·03	29·45	29·45	57·3	67·3	56·0	66	52	61	62	50	49½	43
3.	29·486	29·575	29·467	28·90	29·20	29·44	29·46	29·72	59·2	66·3	53·8	68	43	59·0	50½	45	40½	40½
4.	29·700	29·639	29·223	29·14	29·63	29·73	29·93	30·05	62·5	69·3	52·2	68	38	61	53½	42	45	42
☽ 5.	29·850	29·785	29·780	29·25	29·74	29·77	30·06	30·08	57·3	71·3	48·5	68	39	59	48½	41½	48	45
6.	29·840	29·861	29·789	29·31	29·80	29·79	30·04	29·97	56·2	79·0	48·4	60	43	59	55	41	49	47
7.	29·950	29·806	29·671	29·27	29·67	29·30	29·75	29·52	56·7	66·3	47·6	64	50	56·5	57	45	49	44
8.	29·454	29·407	29·256	28·86	28·96	28·85	29·09	29·03	58·7	61·2	51·7	65	50	60	58	49	51	49
9.	29·422	29·642	29·346	28·82	28·90	29·21	29·06	29·23	60·3	67·7	53·0	63	49	58	58	48	50	49
10.	29·816	29·937	29·749	29·24	29·50	29·80	29·45	29·85	58·8	65·3	51·8	64	47	57	59	49	49	48
11.	30·108	30·045	30·040	29·57	......	30·03	30·03	30·18	55·3	66·8	50·0	64	48	57	61½	...	49	48
○ 12.	30·054	30·014	29·969	29·61	......	30·05	30·26	30·30	54·3	61·4	50·5	67	48	52·5	59	...	50	47
13.	29·878	29·643	29·792	29·47	30·03	30·00	30·30	30·32	49·7	58·8	49·5	56	49	52·5	63	44	50	49
14.	29·970	29·974	29·967	29·56	29·98	30·05	30·33	30·33	58·7	60·0	50·5	69	57	63	71	45	54	52
15.	30·036	29·969	29·959	29·53	30·08	30·08	30·33	30·32	63·3	65·4	58·2	71	48	63·5	73	47½	63	56
16.	30·000	29·900	29·926	29·49	30·04	30·02	30·26	30·23	63·7	72·6	53·6	71	46	66	76	51	63	54
17.	30·058	29·979	29·972	29·57	30·00	29·96	30·20	30·16	55·3	69·8	50·4	75	50	56	76	43½	52	53
18.	29·986	29·939	29·823	29·52	29·92	29·89	30·12	30·12	54·8	71·2	52·0	65	50	57	73	46	53	49
☾ 19.	29·878	29·954	29·824	29·48	29·93	30·00	30·12	30·18	53·5	70·0	51·0	63	59	54	73	46	50½	50
20.	30·122	30·160	29·936	29·62	30·08	30·04	30·18	30·01	51·7	63·2	51·0	63	39	52·5	67½	47	56	51
21.	30·178	30·128	29·984	29·45	29·91	29·90	29·82	29·92	59·3	62·0	50·0	73	51	62·5	65	50½	53½	50
22.	30·044	30·045	29·984	29·47	29·95	29·98	30·00	30·08	60·8	72·6	56·6	70	40	65	48	45	51	50
23.	30·126	30·061	30·027	29·56	30·00	29·99	30·08	30·12	61·3	72·8	51·3	72	49	64	72	47	53½	51
24.	30·070	30·011	29·987	29·55	29·98	29·99	30·13	30·17	59·3	70·3	54·0	66	48	60	74½	49	51	51
25.	29·986	29·962	29·906	29·49	29·99	29·94	30·17	30·16	52·5	65·4	51·0	65	41	55	63	47	55	52
26.	29·940	29·888	29·845	29·42	29·89	29·83	30·08	30·04	56·4	64·0	49·3	73	43	58·5	67	41	51½	50
● 27.	29·830	29·776	29·676	29·31	29·77	29·75	30·00	29·93	64·3	79·7	53·7	76	49	58	65	44	50	49
28.	29·674	29·693	29·629	29·20	29·68	29·70	29·65	29·83	54·3	73·6	51·5	63	40	53	61	42	50	49
29.	29·810	29·797	29·754	29·33	29·76	29·69	29·73	29·71	56·6	63·3	47·3	65	48	53	64	42	51	51
30.	29·922	29·918	29·873	29·36	29·68	29·70	29·70	29·75	60·3	64·3	55·2	67	49	57	61	50	52	49
Mean.	29·875	29·860	29·761	29·33	29·738	29·760	29·923	29·944	57·8	67·6	51·9	66·57	47·13	58·4	63·6	45·2	51·28	46·81

Days of Month. 1843. June.	Wind. London: R.S. 9 a.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. London: R.S. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.	Dew-point. Lond. R. S. 9 a.m.
1.	s.	sw.	calm	e.	ese.	·086	·03	·05	......	......	56
2.	e.	sw.	se.	sse.	nne.	·041	·08	......	......	·04	58
3.	s.	sw.	sw.	ne.	n.	·061	......	·19	......	·57	55
4.	e.	s.	calm	ne.	n.	......	·02	·06	......	......	56
☽ 5.	s.	s.	calm	e.	ne.	·033	·09	1·10	......	......	52
6.	se.	w.	calm	n.	ne.	·130	·17	......	2·06	......	48
7.	s.	sw.	nw.	s.	n.	·247	·23	......	......	......	52
8.	s var.	sw.	w.	s & se.	se.	·130	·10	·22	......	·13	54
9.	s.	sw.	w.	w.	e.	·033	·01	·07	......	......	54
10.	w.	nw.	w.	nw.	n.	·055	·34	......	......	·07	53
11.	w.	w.	w.	n.	nw.	·047	......	......	......	......	50
○ 12.	n.	ne.	ne.	nw.	e.	......	·20	·04	......	·03	52
13.	nnw.	ne.	ne.	nw.	e.	·211	·14	......	......	......	51
14.	n.	se.	se.	ne.	se.	·119	·18	·05	0·40	......	54
15.	ne.	e.	se.	e.	e.	·016	......	......	......	......	59
16.	n.	ne.	e.	ne.	ne.	......	......	......	......	......	56
17.	n.	ne.	e.	ne.	n.	......	......	......	......	......	54
18.	ne.	e.	calm	ne.	n.	......	·01	......	......	......	56
☾ 19.	n.	ne.	e.	ne.	n.	......	·01	......	......	......	55
20.	n.	ne.	ne.	s.	w.	......	......	......	......	......	48
21.	w.	sw.	w.	w.	w.	......	......	......	......	·04	58
22.	n.	ne.	calm	sw.	nw.	......	......	......	0·00	......	55
23.	n.	e.	calm	s.	w.	......	......	......	......	......	55
24.	n.	ne.	calm	e.	n.	......	......	......	......	·04	55
25.	nnw.	n.	e.	e.	ne.	......	......	......	......	......	52
26.	n.	e.	calm	ene.	nne.	......	......	......	......	......	52
● 27.	e.	ne.	calm	ne.	n.	......	......	......	......	......	53
28.	n.	n.	nw.	ne.	n.	......	·01	......	......	......	45
29.	nw.	nw.	w.	nw.	nw.	·016	......	......	0·00	......	42
30.	w.	w.	w.	w.	n.	......	......	......	......	·12	54
Mean.						Sum. 1·225	1·62	1·78	2·45	1·04	Mean. 53

THE
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[THIRD SERIES.]

SEPTEMBER 1843.

XXI. *On the Decomposition of Carbonic Acid Gas and the Alkaline Carbonates, by the Light of the Sun; and on the Tithonotype.* By JOHN W. DRAPER, *M.D., Professor of Chemistry in the University of New York**.

FOR many years it has been known that the green parts of plants under the influence of the sunlight possess the power of decomposing carbonic acid and setting free its oxygen. It is remarkable that this, which is a fundamental fact in vegetable physiology, should not have been investigated in an accurate manner. The statements met with in the books are often far from being correct. It is sometimes said that pure oxygen gas is evolved, that the decomposition is brought about by the so called "chemical rays;" these, and a multitude of other such errors pass current. So far as my reading goes no one has yet attempted an analysis of the phænomenon by the aid of the prism, the only way in which it can be truly discussed.

In a paper by Dr. Daubeny, inserted in the Philosophical Transactions for 1836†, two facts which I shall verify in this communication are fully established. These are,—1st, the constant occurrence of nitrogen gas in mixture with the oxygen, an observation originally due to Saussure, or some earlier writer; and, 2nd, that the act of decomposition is due to the LIGHT of the sun. This latter result, obtained by employing coloured glasses or absorbent media, has not been generally received. Doubt will always hang about results obtained in this way, and nothing but an analysis by the prism can be satisfactory. It has happened, therefore, in books of credit published since that time, that other interpretations of the phænomena have been given. (Johnston's Agr. Chem., Lect. 5. § 7.) (Graham's Chem., p. 1013.)

* Communicated by the Author.

† Noticed in Phil. Mag. S. 3. vol. viii. p. 416.—EDIT.

In its connexions with modern organic chemistry and physiology the experiment of the decomposition of carbonic acid by leaves assumes extraordinary interest. To no other single experiment can the same importance be attached. When we remember that this decomposition is the starting-point for organization out of dead matter, that commencing with this action of the leaf the series of organized atoms goes forward in increasing complexity, and blood, and flesh, and cerebral matter are at its terminus, it is clear that unusual importance belongs to precise views of this the commencing change. The beams of the sun are the authors of all organization.

There is but one way by which the question can be finally settled, and that is by conducting the experiment in the prismatic spectrum itself. When we consider the feebleness of effect which takes place, by reason of the dispersion of the incident beam through the action of the prism, and the great loss of light through reflexion from its surface, it might appear a difficult operation to effect a determination in this way. Encouraged, however, by the purity of the skies in America, I made the trial and have met with complete success.

Before entering on the experiments which I have to communicate, I cannot avoid once more impressively calling the attention of chemists to the true character of those emanations which are here designated "tithonic rays." It is not enough that we admit the existence, throughout the spectrum, of dark rays, possessing the power of bringing about chemical changes; it is not enough that we call them chemical rays; there are qualities of distinction appertaining to them which mark them out as being specific in their kind, properties which they possess totally distinct from those of light and heat. Their title to the rank of a distinct imponderable agent is just as perfect as that of light or heat. From heat they are to be distinguished by incapacity for metallic conduction, and by want of the power of expanding bodies; from light by failing to give any impression to the organ of vision. According to the recognized rules of chemistry they ought to be received as a fourth imponderable agent.

It is not sufficient, as has been said, to call them "chemical rays." The term implies that the distinctive characteristic pertaining to them is the power of changing the composition of bodies. But do not the rays of heat eminently produce like changes? Are not half the decompositions in chemistry brought about by the action of caloric? As respects LIGHT, many instances are already known in which it produces decompositions and combinations; as will be presently shown,

it is the agent that brings about the decomposition of carbonic acid. The faculty of producing like effect is not the distinguishing quality of the tithonic rays, nor can the term *chemical* be any more applied to them than to either of their acknowledged distinct companions. *Unless therefore chemists are content to admit that a species of heat may exist devoid of the power of expanding bodies, of giving the sensation of warmth, and of being transmitted by conducting processes*; *or, unless they admit that light can exist in such a modified condition as to produce in our eyes the sensation of darkness, they will have to admit these tithonic rays as constituting a fourth imponderable agent.* The name they may take is not a matter of importance, that which is least trammelled by hypothesis is best. It is not the object of the papers I have written in this Journal to show merely that a class of invisible rays exists in the spectrum; that has been known for a long time; but it is to point out the true relation of these rays to other bodies and other forces in the world, to assert for them their title of a fourth distinct imponderable agent, and to secure for them the admission of that title by giving them a name.

When the leaves of plants are placed in water from which all air has been expelled by boiling, and exposed to the sun's rays, no gas whatever is evolved from them. When they are placed in common spring or pump water bubbles quickly form, which when collected and analysed prove to be a mixture of oxygen and nitrogen gases; from a given quantity of water a fixed quantity of air is produced. When they are exposed in water which has been boiled and then impregnated with carbonic acid, the decomposition goes on with rapidity, and large quantities of gas are evolved.

The obvious inference which seems to arise from these facts is, that all the oxygen collected is derived from the direct decomposition of carbonic acid. We shall presently examine whether this is the correct inference.

Having, by long boiling and subsequent cooling, obtained water free from dissolved air, I saturated it with carbonic acid gas. Some grass leaves, the surfaces of which were carefully freed from any adhering bubbles or films of air by having been kept beneath carbonated water for three or four days, were provided. Seven glass tubes, each half an inch in diameter and six inches long, were filled with carbonated water, and into the upper part of each the same number of blades of grass were placed, care being taken to have all as near as could be alike. The tubes were inserted side by side in a small pneumatic trough of porcelain. It is to be particularly remarked that the blades were of a pure green aspect as seen

in the water; no glistening air-film, such as is always on freshly-gathered leaves, nor any air bubbles were attached to them. Great care was taken to secure this perfect freedom from air at the outset of the experiments.

The little trough was now placed in such a position that a solar spectrum, kept motionless by a heliostat and dispersed by a flint glass prism in a horizontal direction, fell upon the tubes. By bringing the trough nearer to the prism or moving it further off, the different coloured spaces could be made to fall at pleasure on the inverted tubes. The beam of light was about three-fourths of an inch in diameter. In a few minutes after the commencement of the experiment, the tubes on which the orange, yellow, and green light fell, commenced giving off minute gas bubbles, and in about an hour and a half a quantity was collected sufficient for accurate measurement.

The gas, thus collected in each tube, having been transferred to another vessel and its quantity determined, the little trough with all its tubes was freely exposed to the sunshine. All the tubes now commenced actively evolving gas, which when collected and measured served to show the capacity of each tube for carrying on the process. If the leaves in one were more sluggish or exposed a smaller surface than the others, the quantity of gas evolved in that tube was correspondingly less. As may be readily supposed, I never could get tubes so arranged as to act *precisely* alike, but after a little practice I brought them sufficiently near to equality. And in no instance was this testing process of the power of each tube for evolving gas omitted after the experiment in the spectrum was over.

Table of the Decomposition of Carbonic Acid by Light of different Colours.

Experiment 1.		Experiment 2.	
Name of Ray.	Volume of Gas.	Name of Ray.	Volume of Gas.
Extreme red	·33	Extreme R. and red	·00
Red and orange...	20·00	Red and orange ...	24·75
Yellow and green	36·00	Yellow and green...	43·75
Green and blue...	·10	Green and blue	4·10
Blue	·00	Blue	1·00
Indigo	·00	Indigo	·00
Violet..............	·00	Violet	·00

From this it appears, that the rays which cause the decomposition of carbonic acid gas have the same place in the spectrum as the orange, the yellow, and the green,—the extreme

red, the blue, the indigo, and the violet, exerting no perceptible effect. This being the case, we should expect that by passing a beam through absorbent media of such a nature that the extreme red, the blue, the indigo, and violet are absorbed, this decomposition should nevertheless go on. A solution of bichromate of potash nearly fulfils these conditions, and not only does it absorb the luminous rays in question, but also all the tithonic rays, except a trace of those which correspond to the more refrangible yellow and less refrangible green.

A remarkable proof of the correctness of the foregoing prismatic analysis comes out when leaves are made to act on carbonated water in light which has passed through a solution of bichromate of potash. I took a wooden box of about a cubic foot in dimensions, and having removed its bottom, adjusted to it a trough made of pieces of plate glass. The box being set on end, its lid served as a door, and the trough being filled with a solution of bichromate of potash, the sun's beams came through it, and in the interior of the box an arrangement of leaves and carbonated water could be exposed to the rays that had escaped absorption. The thickness of the liquid stratum was about half an inch. I had several such boxes made, so that I might compare the simultaneous effect of light which had undergone absorption by different media. They formed, as it were, a series of little closets in which bodies could be exposed to party-coloured light—blue, yellow, red, &c.

Whenever an experiment was commenced in these closets, simultaneously a similar one was commenced in the unobstructed sunshine. It is needless to repeat, that in all these care was taken to have the different arrangements for decomposition as nearly alike as possible.

On comparing together the amount of gas evolved in unabsorbed light and in light that had undergone absorption by the bichromate of potash, in three out of five trials the gas collected under the latter circumstances exceeded in volume that collected under the former; this was probably due to a slightly higher temperature which obtained in the box.

On comparing together the volumes of gas collected under the bichromate of potash and under litmus water, the latter was not equal to one-half the former.

I compared together the gas evolved in unobstructed light, under bichromate of potash, and under ammonio-sulphate of copper; the results were as follows:—

Unobstructed light	4·75
Bichromate of potash	4·25
Ammonio-sulphate of copper	·75

Comparing these experiments, made by the aid of absorptive media, with those made by the prism, we are enabled to come to a definite conclusion as to the character of the rays which cause this decomposition.

The true office of prismatic analysis is to determine the refrangibility of the rays which produce given actions; but inasmuch as rays of heat, rays of light, and tithonic rays are found throughout the spectrum, in many cases the prism fails to indicate to which of these imponderable agents phænomena are to be ascribed. The case before us furnishes a striking example. Although the decomposition of carbonic acid is most energetically brought about by rays whose index of refraction corresponds to the yellow, yet that region of the spectrum is far from being devoid of heat and tithonicity.

By considering however the prismatic analysis and the absorptive analysis together, the following facts appear:—1st, the place of maximum action in the spectrum corresponds to the maximum of illumination; 2nd, at the place of the maximum of heat (which in the prism here used is beyond the extreme red) no decomposition whatever takes effect; this appears therefore to exclude calorific influence; 3rd, the point of maximum action of the tithonic rays, which escape absorption by the bichromate of potash, being towards the green, does not correspond with the place of maximum decomposition, which is the yellow; this seems to exclude the tithonic rays; 4th, the decomposition taking place almost as energetically under the bichromate of potash as in the unobstructed beams of the sun, and that salt absorbing all but a mere trace of the tithonic rays, if the effect was due to them it ought to be retarded to an extent corresponding to their loss by absorption, which is far from being the case; the retardation which is observed appearing to be attributable rather to the loss of light by reflexion from the faces of the trough, and the partial turbidity (want of translucence) of its glasses and solutions.

For these reasons I conclude that the decomposition of carbonic acid by the leaves of plants is brought about by the rays of LIGHT; and that the calorific and tithonic rays do not participate in the phænomenon. As was stated before, therefore, the rays of light are just as much entitled to the appellation of chemical rays as those which have heretofore passed under that name.

I might observe in passing, that there is a degree of precision attached to results of the decomposition of carbonic acid which is wholly wanting in most similar experiments. In the stains on Daguerreotype plates, or on photographic papers, though there is no difficulty in ascertaining the place of

maximum effect, yet nothing in the shape of absolute measures of quantities can be obtained. When however gas can be collected and its volume determined, as in the voltameter and in the experiments just described, the results possess a degree of exactness which enables us to draw from them definite conclusions.

Let us now proceed to determine the constitution of the gaseous mixture given off during their decompositions. It is not pure oxygen, as has often been supposed and often disproved, but a mixture of oxygen, nitrogen and carbonic acid. It is mainly to the ratio of the two former that attention has to be directed, the amount of the latter is always variable in different trials. Before proceeding to this there are certain observations to be premised, the results of which, though familiar to chemists accustomed to gaseous analysis, deserve a place here, for they seem to be wholly overlooked in many of the experiments connected with the so-called respiration, but rather digestion, of plants recorded in the books of botany.

When gas of any kind is confined over water in the pneumatic trough, its constitution is undergoing incessant change. A portion of it dissolves more or less slowly in the water, and in exchange it receives from the water gas which is always dissolved therein. If two jars, filled with different gases, stand side by side on the shelf, each is incessantly disturbing the constitution of the other, nor does this disturbance cease until the contents of both jars are chemically the same. There are some beautiful experiments of easy repetition which serve to show how rapidly gases and vapours can thus percolate through fluids. Take a pint bottle and pass through its cork, which ought to fit it very loosely, a glass tube a foot long, drawn narrow at its upper end. Into the bottle put a few drops of water of ammonia. Dip the wide end of the tube into a solution of soap, and introduce it into the interior of the bottle, adjusting it in such a position by the cork, that when air is blown in at the narrow end, the soap bubble which expands at the wide end may occupy the middle of the bottle. Placing the lips on the narrow end, blow a bubble an inch or more in diameter, and, without loss of time, cautiously draw back again the air from the interior of the bubble into the mouth. A strong ammoniacal taste is at once perceived. Now it is obvious that this ammonia must have passed with very great rapidity through the bubble.

A still more instructive experiment may be easily made. Take a three-ounce bottle with a wide neck, close the mouth of it by a film of soap water, by passing the moistened finger over it. Place it under a jar of protoxide of nitrogen. In-

stantly the horizontality of the film is disturbed; it swells upwards, and is spontaneously expanded by the passage of the gas through it into a bubble. The play of colour which attends this experiment, and the excessive thinness which the film finally assumes, render this one of the most beautiful experiments that chemistry can furnish; for when the bubble is almost invisible by reason of its incapacity to reflect light, and can only be seen in particular positions, it still discharges its percolating function.

This percolation of gases through liquids cannot be hindered by employing oil or such other liquids as botanical writers seem to imagine. Through common lamp oil, through copaiva balsam, &c., hydrogen gas will escape with rapidity, and protoxide of nitrogen and carbonic acid still faster. The law that regulates these phænomena is a very simple one,—the gas escapes through the confining medium with a rapidity proportional to its solubility therein.

These things being understood, it is obvious that when carbonic acid is decomposed in the experiments we have been detailing, a variable proportion of that gas will intermingle with the oxygen collected. The proportions must be variable, for it depends on the amount of carbonic acid remaining behind in the water, on the speed with which the experiment is conducted, and other variable conditions. As before stated, therefore, I shall leave out of consideration this carbonic acid, in discussing the analysis of the collected gases, because it is present by accident and is not essentially connected with the phænomena, except in one instance, where dark heat is to be employed, as will be described presently.

Analysis of Air evolved from Carbonated Water by the Sun.

Exp.	Name of Plant.	Oxygen.	Nitrogen.
1.	Pinus tæda	16·16	8·34
2.	do.	27·16	13·84
3.	do.	22·33	21·67
4.	Poa annua	90·00	10·00
5.	do.	77·90	22·10

I may remark that this table contains a few out of a great number of experiments, all of which might have been quoted as examples of the observations which I wish to deduce from it. 1st. They all coincide in this respect, that the oxygen is never evolved without the simultaneous appearance of nitrogen. 2nd. That when certain leaves are employed, as those of the *Pinus tæda*, there seems to be a very simple relation between the volumes of oxygen and nitrogen. In the first

and second of those experiments the volume of the oxygen is to that of the nitrogen as two to one; in the third as one to one. In certain cases this apparent simplicity of proportion is departed from; but from its frequent occurrence in many analyses I have made, it seems to demand attentive consideration. Moreover, in other plants, as in experiments 4 and 5, the amount of oxygen is relatively greater, and between it and the nitrogen there does not appear any exact proportion.

In order to ascertain whether decompositions taking place under absorbent media, as bichromate of potash, produce the same results as indicated in the foregoing table, I made several analyses of gas collected under these circumstances. The presence of the absorbent medium did not seem to exert any influence whatever, the general results coming out as though it had not been employed.

It has long been a matter of popular observation that the sunlight has the quality of extinguishing domestic fires. I do not know whether there is in reality any ground for this opinion; or if so, whether the phænomenon is in any way connected with the relations of light to carbon and oxygen. Popular opinion ascribes the effect to the light and not the heat of the ray. To determine whether radiant heat, unaccompanied by light, had the power of producing decomposition of carbonic acid through the agency of leaves, I placed in the focus of a large brass concave mirror a vessel containing some pine leaves in carbonated water. The mirror was set before a wood fire, and after a little time the leaves began evolving bubbles. The temperature of the water rose as high as 140° Fahr., and when sufficient gas was collected, examination proved that nearly the whole of it was absorbed by lime or potash water. From this it is evident that radiant heat merely liberates the carbonic acid, and does not decompose it. This corroborates therefore the result of pneumatic analysis, that it is the light and not the heat which brings about the change.

Decomposition of Alkaline Salts. — The conditions under which carbonic acid gas is decomposed being understood, I pass now to the description of similar decompositions occurring in the case of saline bodies. It has always been a subject of surprise to chemists, that the powerful affinity by which carbon and oxygen are held together should be so easily overcome at common temperatures. Even potassium cannot decompose carbonic acid in the cold. It might therefore be reasonably expected that the energetic forces which bring about this change ought also to effect other remarkable decompositions.

In fact, as I shall now proceed to show, the decomposition of carbonic acid is only one of a very numerous series.

The alkaline bicarbonates, as is well known, undergo decomposition by a slight elevation of temperature. When boiled with water they gradually give off their second atom of acid, and slowly pass into the condition of neutral carbonate. This easy decomposibility led me to inquire whether green leaves, under the action of the sunlight, would effect the liberation and subsequent reduction of the acid. In the following experiments it is to be observed, that the boiling is not continued long enough to affect to any extent the constitution of the salt, and in each case any portion of free carbonic acid extricated during the cooling of the liquid was removed by the action of the air-pump. The solution when finally used contained no gaseous matter, but only the salt dissolved in water.

Having boiled some distilled water to expel all gaseous matter, dissolve in it a small quantity of bicarbonate of soda. Introduce into a test tube some leaves of grass, fill the tube with the saline solution which has been once more boiled to expel any air it may have obtained from the dissolving salt, and invert the tube in some of the solution in a wine glass, after having carefully removed all adhering bubbles of air from the leaves by a piece of wire, or in any other convenient manner. This arrangement kept in the dark undergoes no change; but, if brought into the sunshine, bubbles of gas are rapidly evolved, and in the course of a few hours the tube becomes half-full. On detonation with hydrogen this gas proves to be rich in oxygen.

I made some attempts to discover how much oxygen could in this way be evolved from known quantities of bicarbonate of soda, supposing it probable that the second atom of carbonic acid being removed and decomposed, the process would cease. I need not detail the result of those trials; they indicated that the supposition I had formed was not correct. The process is not limited to the removal and decomposition of the second atom, but goes forward, the first atom itself being in like manner decomposed. From this it would seem that carbonate of soda itself should be decomposed, and experiment verifies the conclusion; for on using that salt instead of the bicarbonate, the evolution of oxygen goes on precisely in the same way.

As in these experiments *solid* salt dissolved in water is decomposed, it is obvious that the function by which the leaves accomplish this is very different from that of respiration. It is not respiration, but a true digestion.

Liebig has shown that ammonia exists in the ascending sap. It is probable, therefore, that it does not undergo final change before reaching the upper face (sky-face) of the leaf. There, if it be in the form of a carbonate, it unquestionably is concerned in decomposition. With the natural experiment before us, we might expect that the carbonate of ammonia used in place of the soda salts of the last experiment would yield like them. Accordingly it will be found, by using the officinal sesqui-carbonate of ammonia, that leaves effect its decomposition. In numerous experiments it has yielded me gas frequently containing more than 90 per cent. of oxygen.

In every instance which I have examined the gas evolved from leaves is not pure oxygen, but, as has been said, a variable mixture of oxygen and nitrogen. This result is of uniform occurrence; I have observed it in low latitudes where the sun is extremely brilliant, in the case of different plants; and on referring to Dr. Daubeny's paper, it will appear that he has uniformly recognized the same result in England. The very remarkable qualities which certain nitrogenized substances are known to exhibit, acting as ferments as they are undergoing decay, might lead to the suspicion that the decomposition of carbonic acid by leaves is due to the action of some nitrogenized body, the eremacausis of which is promoted by the rays of the sun.

There are many facts which go to prove that the decomposition of carbonic acid is a secondary result brought about by the action of a nitrogenized ferment in a state of eremacausis, the sunlight operating in the first instance upon the ferment itself. Plants can grow in a certain manner in dark places, and if the observations of botanists have been correctly made, although this kind of growth may be abnormal, it eventuates in increasing the total weight of carbon. It signifies little that in these instances lignin may often be deficient, for other bodies of the starch family make their appearance; and results of this kind serve to show that, though in all ordinary cases the union of carbon with the elements of water is an effect of light, there are other cases in which, either by ferment action, or other powers residing in the plant, the same result can be attained.

Boussingault states that grass leaves dried in air at 212° Fahr., and burnt with oxide of copper, yield 1·3 per cent. of their dry weight of nitrogen, which nitrogen is of course in combination. I found, however, that there is besides this, included in the tissue of the leaf, a certain quantity of gas which may be removed by the air-pump. I presume this air is naturally inclosed in the spiral vessels. When leaves are placed in an inverted jar with boiled water *in vacuo*, this gas is li-

berated; at first most copiously from the fractured extremity, but as the process of exhaustion goes on it exudes from both faces of the leaf, perhaps by rending open the frail tissue in which it is imprisoned. In leaves that have stomata on one side only, it does not pour forth from those organs in preference to other parts: and from this it may be inferred that it does not normally exist in the intercellular spaces. In a given weight of leaves its amount is very variable, ranging in my experiments from ·01 to ·02 cubic inch for ten grains of grass leaves. Its constitution as determined by analysis is also variable, but very remarkable; it contains from 88 to 94 per cent. of nitrogen.

It being therefore understood that in the tissue of the leaf a certain quantity of gas is mechanically included, which gas differs from atmospheric air in the circumstance that it contains a larger volume of nitrogen, which may be removed by the air-pump, we are in a condition to understand whether it is this nitrogen which furnishes the supply found in the gas exhaled by leaves. The following experiment proves that it is not.

I removed by continued boiling and exhaustion all the air dissolved in a solution of bicarbonate of soda. I also removed all the nitrogen from some grass leaves, by placing them *in vacuo* immersed in water that had been boiled and subsequently cooled. Then, placing these leaves in the solution of the bicarbonate and in the vessels in which the experiment was finally to be conducted, I kept them *in vacuo* for an hour. This was done to get rid of that film of atmospheric air which always adheres to the surface of glass vessels, and which might have disturbed the result by furnishing nitrogen. The leaves were now exposed in the saline solution to the beams of the sun, and presently the evolution of gas commenced. When a sufficient quantity was collected, it was found to consist of 88 per cent. oxygen and 12 nitrogen.

Repetitions of this experiment prove that although the nitrogen mechanically inclosed in the leaf to a certain extent mingles with the oxygen evolved, and indeed it could not be otherwise on account of the diffusion of gases into one another, yet the true source is to be sought in some nitrogenized compound present in the leaf, which is undergoing decomposition in a regulated way.

Keeping this fact clearly before us, that the source of the nitrogen found thus in company with the oxygen given off under the influence of light is some nitrogenized body existing in the leaf, the following experiments will show the simple and beautiful law under which this phænomenon is conducted.

Saussure has already determined, that when plants are forced to grow in an atmosphere of known volume, containing carbonic acid gas, after the decomposition of the gas is completed, the total volume remains unchanged. As my experiments were made with leaves immersed in water, I was desirous of proving whether under these forced circumstances the same result would still hold good.

To a certain quantity of water, from which all air had been expelled, confined in a jar over mercury, I passed 20 measures of carbonic acid gas; by a little agitation the water took up 15·50 measures of the acid. I now introduced into the jar some leaves, taking the greatest care that no bubbles of air should pass along with them. The jar was then placed in the sunshine, and the decomposition completed. Corrected for variation of temperature and pressure, the resulting volume of the gas in two experiments was 20, or precisely the same as that of the carbonic acid.

We may therefore infer that the volume of mixed gases evolved is precisely equal to the volume of carbonic acid that disappears. This leads us to some very remarkable conclusions.

When the leaves of plants under the influence of light decompose carbonic acid, they assimilate all the carbon, and a certain proportion of oxygen disappears, at the same time they emit a volume of nitrogen *equal to that of the oxygen consumed.*

This disappearance of oxygen and appearance of nitrogen are thus connected with each other; they are equivalent phænomena.

The emission of nitrogen is thus shown not to be a mere accidental result, but to be profoundly connected with the whole physiological action.

I arrive also at this conclusion from experiments of another kind. If the nitrogen that appears in company with oxygen were obtained by diffusion from gas mechanically shut up in the parenchyma of the leaf, it is plain, in the mode of operation which I have followed, in which leaves are immersed under water, and no opportunity given them of restoring their mechanically included air, if it were by any means withdrawn, that the first portions of mixed gas evolved should be richest in nitrogen, and that the per-centage amount should gradually become less and less, as it was removed from the structure of the leaf; this follows from the laws of the diffusion of gases. But this is far from being the case. It very commonly happens that more nitrogen is evolved *at the close of the process* than at its beginning. Thus, in one of the experiments I made, in which it was found that there was 22·2 per cent. of nitrogen

in the total resulting volume, the quantities that had been evolved in three successive periods of examination from the beginning to the termination of the experiment were,

1st period,	21·8	per cent. of nitrogen.	
2nd ...	18·8	...	
3rd ...	26·0	...	

During the progress of this decomposition, therefore, more nitrogen relatively was evolved towards the close of the experiment than at its beginning.

From this result, therefore, I again infer that the nitrogen emitted by leaves is derived from the decomposition of some azotized body, and not from air mechanically included in their pores.

The following are the experimental results which I have obtained:—

1st. That the nitrogen comes from the tissue of the leaf itself; because more than three times as much is evolved from bicarbonate of soda as is imprisoned in the structure of the leaf, removable by the air-pump.

2nd. In twelve hours, from bicarbonate of soda, leaves will evolve more than five times their own volume of gaseous matter.

3rd. The quantity of nitrogen in the composition of leaves is sufficient for furnishing all the nitrogen obtained in the gas evolved. From Boussingault's analyses it appears that they contain nearly ten times the required amount.

4th. The decomposition of some nitrogenized constituent of the leaf is essential to the appearance of the nitrogen; there is no other available source.

At this stage of the inquiry a remarkable analogy appears between the function of digestion in animals, and the same function in plants. Liebig has shown how, from the transformation of the stomach itself, food becomes acted upon and is turned into chyme; an obscure species of fermentation brought about by the action of nitrogenized bodies. So, in like manner in plants, the decay of a nitrogenized body is intimately connected with the assimilation of carbon, for, as I have stated, the process here under discussion is a true digestion, and not a respiratory process. And as there are facts which seem to show that the primary action of the light is not upon the carbonic acid, but upon the nitrogenized ferment, the decomposition of the gas ensuing as a secondary result, *is it not probable that* CHLOROPHYL *is the body which in vegetables answers to the* CHYLE *of animals?* The oxygen, which disappears during the decomposition of carbonic acid, disappears to bring about the eremacausis of the nitrogenized body. And have not the gum, the starch, the lignin, and other car-

bonaceous constituents of plants, all originally existed in and passed through the green stage? It is the quality of radiant matter to determine the position of atoms and the grouping of molecules; and for this the sun, the great organizer, the great life-giver, from age to age furnishes his unfading beams. That analogies like this between the organic functions of plants and animals in reality exist we might reasonably suppose; they are agreeable to the general plan of nature.

Note on the Tithonotype.

In the Number of this Journal for May last, I described a process for obtaining tithonotypes, or copies of the surface of Daguerreotypes by means of gelatine.

A very important improvement on that process, an improvement which, indeed, has brought it almost at once to perfection, has been effected;—this is, *to copy the surface in copper by the Electrotype after it has been previously fixed by the agency of a film of gold.*

Those who are conversant with these matters will see at once that this is a very different thing from the abortive attempts which were made early in the history of the Daguerreotype. Many artists endeavoured to transfer its surface by precipitating copper upon it; among others I made trials of the kind. The results of those abortive attempts were mere shadowy representations which could be seen in certain lights, and which were very unsatisfactory in their effect*.

The beautiful tithonotypes that are now so common in this city are made in the following way:—The Daguerreotype is carefully gilt by M. Fizeau's process, taking care that the film of gold is neither too thick nor too thin. The proper thickness is readily attained after a little practice. The plate is then kept a day or two, so that it may become enfilmed with air. The back and edges being varnished, copper is to be deposited upon it in the usual way, the process occupying from twelve to twenty hours. If the plate has been properly gilt, and the process conducted successfully, the tithonotype readily splits off from the Daguerreotype.

The reader will understand, that when the process succeeds the Daguerreotype will be uninjured, and the Tithonotype a perfect copy of it. If any portions are blue, or white, or flesh-coloured, they will be seen *in the same colours* in the tithonotype; the intensity of light and shadow is also given with accuracy, and indeed the copy is *a perfect copy* in all respects of the original. A great advantage is also obtained

* Professor Grove's voltaic process for etching Daguerreotypes, has, however, produced better results than those here alluded to by Dr. Draper. See Phil. Mag. S. 3. vol. xx. p. 18.—Edit.

in the reversal that takes place. The right side of the tithonotype corresponds to the right side of the original object, and the left to the left. In the Daguerreotype it is not so.

Copper tithonotypes were first made in this city by Mr. Endicott, a lithographic artist of distinction.

There is no great difficulty in obtaining from these tithonotypes duplicate copies. An expert artist can multiply them from one another.

The problem of multiplying the beautiful productions of M. Daguerre is therefore solved.

I will take this opportunity of making a remark which I intended to have inserted in my paper "On the rapid Detithonizing Power of certain Gases and Vapours," inserted in the March Number of this Journal (S. 3. vol. xxii.). Amateurs, in the Daguerreotype process, are often annoyed by the want of success which frequently attends them. They ascribe to the atmosphere, or to the light, or to other causes, their inability to obtain impressions. Most of these mischances are due to the accidental presence of the vapour of iodine, or other electro-negative bodies, in the chamber or about the apparatus. It is incredible what a brief exposure to these vapours will entirely destroy a picture before it is mercurialized. If the iodine box or the bromine bottle is kept in the same room with the mercury apparatus, that circumstance in itself is often sufficient to ensure an uniform want of success. If the little frame which fits into the back of the camera, and which holds the silver plate, be used in the iodizing process, as is often the case, the small quantity of vapour it absorbs will destroy every picture, or at all events increase the time required in the camera enormously. The reason of this is easily understood. Suppose a plate, in such a frame, be placed in the camera, or what comes to the same thing, suppose a particle of iodine has fallen into the camera, or that the wood has in any way absorbed an electro-negative vapour; as fast as the light makes its impression on the sensitive surface the vapour detithonizes it, and unless the light is quite intense or the exposure much prolonged, a very feeble proof, or no proof at all, will be obtained. In the same way the difficulties are greatly increased in the process of mercurialization, for the temperature resorted to being high, if there is the least particle of iodine about the box, the picture will be inevitably and instantly detithonized and ruined.

We ought therefore never to allow iodine, or bromine, or chlorine, to have access to the apartment or the apparatus in which Daguerreotype operations are being conducted.

University of New York, May 20, 1843.

XXII. *On the Use of Lightning-Conductors in India, with reference to a passage in* Mr. Snow Harris's *work on Thunder-Storms. By* W. B. O'Shaughnessy, *M.D., F.R.S., Hon. East India Company's Service.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the work just published by Mr. Snow Harris on "Thunder Storms," there appears at p. 177, an assertion that "the Governor-General and Council of the Honourable East India Company were led to order the lightning rods to be removed from their powder magazines and other public buildings, having in the year 1838 come to the conclusion, from certain representations of their scientific officers, that lightning rods were attended with more danger than advantage." Mr. Harris then refers in a note to Correspondence between the Honourable Court of Directors, Mr. Daniell, and myself, as his authority for this statement.

As this assertion of Mr. Harris is altogether unfounded on fact, and is moreover calculated to do me serious injury, I beg permission, as a subscriber of many years to your excellent Journal, to offer proof in your pages that no such measures as those specified by Mr. Harris were ever recommended or carried into effect in Bengal by the parties, or at the time he mentions.

I inclose (No. 1) a copy of the report by the civil architect of Fort William, showing that in 1838, by my suggestion and advice, additional conductors were erected on and around the Government House of Calcutta.

I also inclose a copy of a note with which I have been favoured by Lord Auckland, showing that no orders were given to remove the conductors from *public* buildings in India; that the powder magazines in that country had not been provided with conductors previously to 1840; and that the only discussion which occurred there, was as to the number and position of the rods to be erected for the protection of such buildings. I have the honour to be, Gentlemen,

Your obedient Servant,

W. B. O'Shaughnessy, M.D., F.R.S.,
H.E.I.Co.'s Service.

Upper Bagot-street, Dublin,
Aug. 7, 1843.

(No. 1.)

Report by Captain Fitzgerald *on the accident by Lightning to Government House, Calcutta.*

"Fort William, 30th March, 1838.

"Sir,—I have the honour to report for the information of the Military Board, that the Government House was struck by

lightning during the storm which occurred early this morning. The lightning seems to have been attracted to the building by the iron at the point of the spear attached to the figure of Britannia on the top of the dome; after demolishing the spear, it pursued its course down the external copper of the dome, without apparently doing any injury, and forced its way into the ball-room in three separate places. It has left its traces on the ceiling and wall of the southern division of the room, where it has injured one of the pier-glasses, and then passed out at the adjoining window. Again, on the eastern side of the central division it has pursued a similar course, injuring a pier-glass, and again passing out of the adjoining windows. On the western side of the central division it has done the most injury, for after passing through the ceiling it has broken one of the pier-glasses at its corner, then running down into the marble hall, has escaped out of one of the windows, breaking in its exit, as the others also did, several panes of glass.

"2nd. I requested Dr. O'Shaughnessy to inspect the effects of the lightning, and he has expressed his surprise that so little comparative injury has been caused by it. The sharp point of iron at the end of the spear, and the studding of the shoulders of the statue with iron nails (intended to prevent birds from sitting on it), has served in the first instance to attract the lightning, and that it has never been struck before, he attributes to the protecting power of the four conductors, which, however, he considers to be twice as far from each other as they ought to be.

"3rd. In repairing the statue, he recommends that the spear should be made of metal, and that it should be connected with one or more of the corner conductors by means of a continuous metallic rod. It would perhaps also be advisable under the circumstances above mentioned, to affix four more conductors to the house, to render it more secure from a similar visitation.

"4th. With the Board's permission, I will, in rectifying the damage, carry the improvements above suggested into effect.

"I have, &c.

(Signed) "W. R. Fitzgerald,
Civil Architect."

"*To Captain Sanders,*
Secretary, Military Board."

(No. 2.)

Note from the Earl of Auckland, late Governor-General of India.

"Kensington Gore, July 27, 1843.

"Dear Sir,—I can have no objection to giving you the information which you request from me, though I would do so

very generally, for I would not trust to my recollection, and would have you, if you wish for perfect accuracy, to refer to the India House for a detailed account of what passed between you and the Government of India on the subject of lightning-conductors.

"I am however certain that there was no question or order on the 'removal' of lightning rods. The proposition discussed was one for erection of lightning-conductors for the safety of all powder magazines; such magazines having hitherto been thought secure from accident without them. And upon this questions arose,—1st, as to the necessity of such erections; and 2ndly, if erected, as to what should be the form and size and distance from each other, and from the magazine, of the conductors. I think that your reasonings upon the danger of small conductors placed very near to a hazardous building was very generally admitted to be convincing. In the end, as it were necessary that the rods, if constructed, should be made in England, it was thought best to refer the whole question to the authorities at home.

"Very faithfully yours,
(Signed) "AUCKLAND."

"*To Dr. W. B. O'Shaughnessy.*"

XXIII. *A new Process for preparing Cyanogen.* By ALEXANDER KEMP, *Esq.**

ON mixing together cyanide of potassium and bichloride of mercury, both in powder, and leaving them for a few days, I observed that the mixture became of a greenish colour, which at first led me to suspect the presence of iron in the bichloride of mercury; but as I failed in detecting it, I next proceeded to make a few experiments with the substances, the result of which was, that I found that cyanogen might be more easily and œconomically obtained by the following method, than by any of the usual processes.

Take six parts perfectly dry ferrocyanide of potassium, and nine parts bichloride of mercury, both in fine powder, and mix them intimately together, then apply heat to the mixture, in a glass retort, when cyanogen gas will be disengaged, mercury at the same time distils over, and a dark-coloured matter is left in the retort, being a mixture of chloride of potassium and cyanide of iron.

University of Edinburgh, Aug. 11, 1843. ALEX. KEMP.

* Communicated by the Author.

XXIV. *On the Geology and Palæontology of North America, in abstracts of a series of papers recently communicated to the Geological Society of London. By* David Dale Owen, *M.D.*; Charles Lyell, *Esq., V.P.G.S., F.R.S.*; Gideon Algernon Mantell, *LL.D., F.R.S.*; W. C. Redfield, *Esq.*; *and* J. Hamilton Cooper, *Esq.*

1. *On the Geology of the Western States of North America.* By David Dale Owen, M.D., of Indiana*.

THE remarks of the author relate chiefly to that part of the western states watered by the rivers Ohio, Wabash, Illinois, Rock, Wisconsin, Cumberland, and Tennessee, lying between 35° and 43° of north latitude, and 81° and 91° of west longitude. It includes the States of Illinois, Indiana, Ohio, Kentucky, Tennessee, and the Dubuque and Mineral Point districts of the territories of Jowa and Wisconsin. The observations recorded are the results of numerous excursions in those provinces, commenced in the year 1834, and continued to the present time by Dr. Owen, sometimes alone, at others accompanied by Dr. Troost and Dr. Locke, the state geologists respectively of Tennessee and Ohio. Though the territory under consideration occupies an area of about half a million of square miles, its geological features are remarkably uniform. With a few partial exceptions its formations belong to the eras of the bituminous coal, the mountain limestone of Europe, and the Silurian rocks of Murchison. The exceptions are the superficial deposits which occasionally cover these up from view, over considerable districts, and which themselves must be referred to the age of the gigantic mammalia and formations of a still more recent date; together with a marl and greensand in the western district of Tennessee, corresponding probably to the greensand and other members of the cretaceous group.

Of the tract described, the formations west of the Tennessee river occupy but a small corner, and the author has had but limited opportunities of examining them in person. The upper part of this group is an argillaceous marl of a light gray colour; the lower (of unascertained thickness) a greenish sandy marl. In no instanee, as far as known to the author, has either the greensand or marl been discovered east of the Tennessee river. But it exists, according to Dr. Troost, under the superficial soil in most of the countries west of that river, extending probably west and south, into the states of Mississippi and Alabama. Both the marl and greensand are rich in fossils. In the former the most characteristic shell is the *Exogyra.* Though it is evident, from the character of the fossils imbedded in the marl and greensand beds, that these belong to the cretaceous group, yet hitherto no true chalk has been discovered in Tennessee, nor, so far as I know, in any of the United States.

In the territory described are two coal-fields of great extent. On the west is the great Illinois coal-field, equalling in area the entire island of Great Britain, occupying the greater part of Illinois, about one-third of Indiana, a north-western strip of Kentucky, and extend-

* Read before the Geological Society, Nov. 2, 1842.

ing a short distance into Jowa. It is covered on the north by extensive diluvial deposits, sometimes to the depth of more than a hundred feet. The other coal-field forms a part of at least six states, viz. Ohio, Kentucky, Tennessee, Pennsylvania, Maryland, and Alabama; and its area is estimated at 50,000 square miles. These coal formations consist, as in Europe, of sandstones, shale, slaty clays, seams of coal, and occasionally beds of limestone, these latter usually dark-coloured and bituminous. At the base of the Ohio formation is a conglomerate from 200 to 300 feet in thickness, which has been referred to the millstone grit of England. A similar conglomerate shows itself in one or two localities at the base of the Illinois coal-field.

The thickness of these coal-fields is estimated at from 1200 to 2000 feet. All the coal is of a bituminous character, some of the caking variety, some splint coal, some cannel. Neither of the coal-fields have suffered much from dislocation; no dykes of trap, whinstone, basalt, or greenstone have been met with in either. On the eastern flank, however, of the Cumberland mountains, the coal is occasionally much disturbed, even thrown up nearly vertically. There is a striking analogy between the fossil flora of these western coal-fields and that of the equivalent strata in Europe. The most productive brines discovered in the western states have been procured by boring through the lower members of the coal measures. Immediately below the coal-formations of Indiana, Illinois, Kentucky, and Tennessee, are limestones mostly of a light gray colour and of a compact texture, including occasionally layers and nodules of chert. Some of these limestones assume the appearance of lithographic stone, others present a beautiful oolitic structure. The strata vary in thickness; in Ohio it does not appear to exist, being replaced by the before-mentioned conglomerate. The great mammoth cave of Kentucky is in the upper beds of this limestone, which abound in subterraneous passages. These beds are characterized by two remarkable fossils, the *Pentremites* and the *Archimedes*, and Dr. Dale Owen has designated the group Pentremital limestones, from the abundance of those fossils. The oolitic stratum lies immediately beneath. No workable seam of coal has hitherto been found beneath the beds containing these fossils; *Productæ* and *Terebratulæ* are abundant in them, and a small species of *Calymene* occurs. Dr. Owen regards these limestones as the equivalent of the mountain limestone of Europe. Iron ores occur at the junction of the limestone and coal measures, and galena and fluorspar have been found in the former.

The rocks which succeed to the Pentremital limestone are gray, yellow, and brown siliceous sandstones, soft and fine grained, sometimes argillaceous and free from mica, passing on the one hand into chert and limestone, and on the other into a rock presenting the appearance of Tripoli: interstratified with these are beds of limestone, occasionally oolitic. This group is not rich in organic remains; *Crinoideæ*, *Polypiferæ* and *Productæ* are most common. The middle and lower beds of this group are regarded by Dr. Owen as probable equivalents of the upper Ludlow rocks.

We next in descending order arrive at a group of bituminous, aluminous shales and associate limestones, the lowest of which affords a valuable water cement. In the shale there are no fossils except a few slight impressions, apparently of seeds or seed-vessels. Where the shale is replaced by indurated clay, Dr. Troost has found Encrinites and Polypifera, and the "encrinital limestone" over the shale in Tennessee is rich in Crinoidea. ***Atrypa prisca, Orthis lunata*** vel ***orbicularis, Terebra sinuosa, Calymene bufo,*** and ***Asaphus macrurus*** occur in the water limestone. The shale, Dr. Owen considers, must probably be referred, as well as the water limestone, to the lower Ludlow, and may be regarded as the equivalent of the Helderberg group and Marcellus shales of the New York geologists. The ***encrinital limestone*** and the green ***ferruginous*** rock of Indiana may correspond with the Aymestry limestones.

Next in order is a group consisting almost wholly of compact limestones, lying in thick beds without any interstratified marls or shales. This rock is best developed towards the north-west, and in certain districts becomes a true magnesian limestone upwards of 500 feet in thickness. It closely approximates, both in lithological character, mineral contents, and even proximity to the coal measures, the "scar limestone" of England, and were it not for the organic remains might be mistaken for it. But, says Dr. Owen, the list of organic remains supplies proof hardly contestable, that the rocks in which they occur are equivalents of the Wenlock formation of Murchison. In the upper beds, ***Catenipora escharoides*** and ***Pentamerus hispidus*** are very abundant, with numerous other species recorded by the author in his memoir. In the lower hundred feet of this group fossils are scarce. Rich and important lead mines occur in it, the most valuable in the United States. The most characteristic fossil of the lead-bearing strata is the ***Coscinopora.***

Next in order follow thin beds of shell limestone, alternating with marl and marlite, occupying a superficial area of about 10,000 square miles. The thickness of this group is greatest about the centre of the Ohio valley, where it is estimated at 1000 feet. In the north-west, at Prairie du Chien, it is but 100 feet, and near the Blue Mounds in Wisconsin, but a few feet in thickness; it abounds in organic remains. Among these are characteristic, ***Isotelus gigas, Triarthrus Bechii,*** several species of ***Conotubularia,*** and of ***Bellerophon*** and ***Maclurites*** ; ***Isotelus planus, Lingula Lewisii, Orthis excentrica, Orthis alata,*** and ***Asterias antiqua.*** These fundamental rocks of the Ohio valley Dr. Owen considers the equivalents of the lower Silurian.

No inferior rocks are visible in a north-west direction until the vicinity of the Wisconsin river, where the blue fossiliferous limestone rests conformably on a sandstone succeeded by a magnesian limestone, with few and imperfect fossils, so that its proper place is doubtful. The blue limestone in the south-east, beyond the Cumberland mountains, rests unconformably on the inferior stratified rocks of Tennessee, which dip ***towards*** the granitic rocks. The author appends extensive lists of fossils.

An extensive series of rocks and fossils from the formations de-

scribed, with beautiful diagrams in illustration of the memoir, were presented to the Society by Dr. Dale Owen at this meeting.

2. *On the Ridges, Elevated Beaches, Inland Cliffs and Boulder Formations of the Canadian Lakes and Valley of the St. Lawrence.* By Charles Lyell, Esq., V.P.G.S., F.R.S.*

After adverting to his former paper on the Recession of the Falls of Niagara, and the observations which he made jointly with Mr. Hall in the autumn of 1841†, Mr. Lyell gives an account of additional investigations made by him in June 1842; in the course of which he found a fluviatile deposit similar to that of Goat Island, on the right bank of the Niagara, nearly four miles lower down than the great Falls. The freshwater strata of sand and gravel here alluded to occur at the Whirlpool. They are horizontal, about forty feet thick, plentifully charged with shells of recent species, and are placed on the verge of the precipice overhanging the river. They are bounded on their inland side by a steep bank of boulder clay, which runs parallel to the course of the Niagara, marking the limit of the original channel of the river before the excavation of the great ravine. Another patch of sand, with freshwater shells, was detected on the opposite or western side of the river, where the Muddy Run flows in, about $1\frac{1}{2}$ mile above the Whirlpool. From the position of these strata it is inferred that the ancient bed of the river, somewhere below the Whirlpool, must have been 300 feet higher than the present bed, so as to form a barrier to that body of fresh water in which the various beds of fluviatile sand and gravel above-mentioned were accumulated. This barrier was removed when the cataract cut its way back to a point further south. The author also remarks, that the manner in which the freshwater beds of the Whirlpool and Goat Island come into immediate contact with the subjacent Silurian limestone, no drift intervening, shows that the original valley of the Niagara was shaped out of limestone as well as drift. Hence he concludes that the rocks in the rapids above the present Falls had suffered great denudation while yet the Falls were at or below the Whirlpool.

Mr. Lyell thinks that the form of the ledge of rock at the Devil's Hole, and of the precipice which there projects and faces down the river, proves the Falls to have been once at that point. An ancient gorge, filled with stratified drift, which breaks the continuity of the limestone on the left bank of the Niagara at the Whirlpool, was examined in detail by the author, and found to be connected with the valley of St. Davids, about three miles to the north-west. This ancient valley appears to have been about two miles broad at one extremity, where it reaches the great escarpment at St. Davids, and between 200 and 300 yards wide at the other end, or at the whirlpool. Its steep sides did not consist of single precipices, as in the ravine of Niagara, but of successive cliffs and ledges. After its de-

* Read Dec. 14, 1842, and January 4, 1843.

† See Proceedings, vol. iii. p. 595 [or Phil. Mag. S. 3. vol. xxi. p. 548].

nudation the valley appears to have been submerged and filled up with sand, gravel, and boulder clay, 300 feet thick.

A description is next given of certain modern deposits, containing freshwater shells, on the western borders of the Niagara, above the Falls, and in Grand Island, in order to show that the future recession of the Falls may expose patches of fluviatile sediment similar to those in and below Goat Island.

The author then passes to the general consideration of the boulder formation on the borders of Lakes Erie and Ontario, and in the valley of the St. Lawrence, as far down as Quebec. Marine shells were observed in this drift at Beauport, below Quebec, as first pointed out by Captain Bayfield, and also near the mouth of the Jacques Cartier river, and at Port Neuf and other places; also at Montreal, where they reach a height probably exceeding 500 feet above the sea, the summit of Montreal mountain being 760 feet high, according to Bayfield's trigonometrical measurement, and the shells being supposed to be 240 feet below the summit. These shells, therefore, being more than 300 feet above Lake Ontario, we may presume that the sea in which the drift was formed extended far over the territory bordering that lake. The most southern point at which the author saw fossil shells belonging to the same group as those of Quebec was on the western and eastern shores of Lake Champlain, viz. at Port Kent and Burlington, in about lat. 44° 30′. Here, and wherever elsewhere the contact of the drift is seen with hard subjacent rocks, these rocks are smoothed, and furrowed on the surface, in the same manner as beneath the drift in northern Europe. The species of shells occurring in the drift, to which Mr. Lyell has made some additions, are not numerous, and are all, save one, known to exist, but are inhabitants, for the most part, of seas in higher latitudes. Many of them are the same as those occurring fossil at Uddevalla and other places in Scandinavia, and they imply the former prevalence of a colder climate when the drift originated. At Beauport there are large and far-transported boulders, both in beds which overlie and underlie these marine shells.

The author next describes the ridges of sand and gravel surrounding the great lakes, which are regarded by many as upraised beaches. He examined, in company with Mr. Hall, the "Lake ridge," as it is called, on the southern shore of Lake Ontario, and other similar ridges north of Toronto, which were formerly explored by Mr. Roy*, and which preserve a general parallelism to each other and to the neighbouring coast. Some of these have been traced for more than 100 miles continuously. They vary in height from ten to seventy feet, are often very narrow at their summit, and from fifty to 200 yards broad at their base. Cross stratification is very commonly visible in the sand; they usually rest on clay of the boulder formation, and blocks of granite and other rocks from the north are occasionally lodged upon them. They are steeper on the side towards the lakes, and they usually have swamps and ponds on their inland side; they are higher for the most part and of larger dimensions

* See Proceedings, vol. ii. p. 537 [or Phil. Mag. S. 3. vol. xi. p. 201].

than modern beaches. Several ridges, east and west of Cleveland in Ohio, on the southern shore of Lake Erie, were ascertained to have precisely the same characters. Mr. Lyell compares them all to the osars in Sweden, and conceives that, like them, they are not simply beaches which have been entirely thrown up by the waves above water, but that many of them have had their foundation in banks or bars of sand, such as those observed by Capt. Grey running parallel to the west coast of Australia, lat. 24° S., and by Mr. Darwin off Bahia Blanca and Pernambuco in Brazil, and by Mr. Whittlesey near Cleveland in Lake Eric. They are supposed to have been formed and upraised in succession, and to have become beaches as they emerged, and sometimes cliffs undermined by the waves. The transverse and oblique ramifications of some ridges are referred to the meeting of different currents and do not resemble simple beaches.

The base-lines of the ridges east and west of Cleveland, are not strictly horizontal according to Mr. Whittlesey, but inclined five feet and sometimes more in a mile. Those near Toronto are said by Mr. Roy to preserve the same exact level for great distances, but Mr. Lyell does not conceive that our data are as yet sufficiently precise to enable us to determine the levels within a few feet at points distant several hundred miles from each other. No fossil shells have been obtained from these ridges, and the author concludes that most of them were formed beneath the sea or on the margin of marine sounds. Some of the less elevated ridges, however, may be of lacustrine origin, and due to oscillations in the level of the land since the great lakes existed, for unequal movements, analogous to those observed in Scandinavia, may have uplifted freshwater strata above the barriers which divide Lake Michigan from the basin of the Mississippi, or Lake Erie from Ontario, or the waters of Ontario from the ocean. Considerable differences of level may have been produced in the ancient beds of these vast inland bodies of freshwater, while the modern deposit and the subjacent Silurian strata may to the eye appear perfectly horizontal.

The author then endeavours to trace the series of changes which have taken place in the region of Lakes Erie and Ontario, referring first to a period of emergence when lines of escarpment like that of Queenstown, and when valleys like that of St. Davids were excavated; secondly, to a period of submergence when those valleys and when the cavities of the present lake-basins were wholly or partially filled up with the marine boulder formation; and lastly, to the re-emergence of the land, during which rise the ridges before alluded to were produced, and the boulder formation partially denuded. He also endeavours to show, how during this last upheaval the different lakes may have been formed in succession, and that a channel of the sea must first have occupied the original valley of the Niagara, which was gradually converted into an estuary and then a river. The great Falls, when they first displayed themselves near Queenstown, must have been of moderate height, and receded rapidly, because the limestone overlying the Niagara shale was of slight thickness at its northern termination. On the further retreat of the sea a second

fall would be established over lower beds of hard limestone and sandstone previously protected by the water; and finally, a third fall would be caused over the ledge of hard quartzose sandstone which rests on the soft red marl, seen at the base of the river-cliff at Lewistown. These several falls would each recede further back than the other in proportion to the greater lapse of time during which the higher rocks were exposed before the successive emergence of the lower ones. Three falls of this kind are now seen descending a continuation of the same rocks on the Genesee River at Rochester. Their union, in the case of the Niagara into a single fall, may have been brought about in the manner suggested by Mr. Hall*, by the increasing retardation of the highest cataract in proportion as the uppermost limestone thickened in its prolongation southwards, the lower falls meanwhile continuing to recede at an undiminished pace, having the same resistance to overcome as at first.

Mr. Lyell considers the time occupied by the recession of the Falls from the Whirlpool to be quite conjectural, but assigns a foot rather than a yard a year as a more probable estimate; thus he shows the Mastodon, found on the right bank near Goat Island, though associated with shells of recent species, to have claim to a very high antiquity, since it was buried in fluviatile sediment before the Falls had receded above the Whirlpool †.

3. *Notice on a Suite of specimens of Ornithoidicnites, or foot-prints of Birds on the New Red Sandstone of Connecticut.* By Gideon Algernon Mantell, LL.D., F.R.S ‡.

These specimens were accompanied by a letter from Dr. James Deane of Greenfield, Massachusetts, the original discoverer of the Ornithoidicnites, of which more than thirty varieties had been found, bearing a striking resemblance to the foot-prints of birds. In this letter Dr. Deane gives an account of his discovery of the impressions eight or nine years ago, and which he then communicated to Professor Hitchcock. He remarks, that "the footsteps are invariably those of a biped, and occur on the upper surface of the stratum, while the cast or counter-impression is upon the lower. In some instances we may follow the progress of the animal over as many as ten successive steps." He has seen a course of steps twelve inches in length by eight in breadth, extending several rods. The intervening space was uniformly four feet. One impression of a foot was fourteen inches in length. The impressions are accompanied by those of rain-drops.

4. *Extract of a Letter from* W. C. Redfield, Esq., *on newly discovered Ichthyolites in the New Red Sandstone of New Jersey.* Communicated by Charles Lyell, Esq., V.P.G.S §.

Mr. Redfield has found two distinct fish-beds in the new red sandstone of New Jersey, both containing ichthyolites of the genus

* Boston Journ. of Nat. Hist., 1841.

† [On the subject of Mr. Lyell's paper, as noticed by Mr. Murchison, see our preceding volume, pp. 548–550.—Edit.]

‡ Read Dec. 14, 1842. See also our preceding volume, p. 557.

§ Read Dec. 14, 1842.

Palæoniscus. In the sandstone between the fish-beds he discovered an Ornithoidicnite, and observed numerous slabs exhibiting impressions of rain-drops and ripple-marks. The rain-marks appear as if the rain had been driven by a strong wind, and the direction of the impressions indicated that the wind blew from the west, a quarter from which violent squalls or thundergusts are still prevalent in these latitudes.

5. *On the Tertiary Strata of the Island of Martha's Vineyard in Massachusetts.* By Charles Lyell, Esq., V.P.G.S., &c.*

The most northern limit to which the tertiary strata bordering the Atlantic have been traced in the United States is in Massachusetts in Martha's Vineyard, lat. 41° 20′ north, an island about twenty miles in length from east to west, and about ten from north to south, and rising to the height of between 200 and 300 feet above the sea. The tertiary strata of this island are, for the most part, deeply buried beneath a mass of drift, in which lie huge erratic blocks of granite and other rocks which appear to have come from the north, probably from the mountains of New Hampshire. The tertiary strata consist of white and green sands, a conglomerate, white, blue, yellow, and blood-red clays and black layers of lignite, all inclined at a high angle to the north-east, and in some of their curves quite vertical. They are finely exposed near Chilmark on the south-west side of the island, and in the promontory of Gay Head at its south-western extremity, where there is a vertical section of more than 200 feet in height.

Attention was first called to this formation by Prof. Hitchcock in 1823, who appears to be the only American geologist who has examined them personally. He compared the beds at Gay Head to the plastic and London clays of Alum Bay in the Isle of Wight, to which, lithologically, they bear a striking resemblance, consisting in both cases of variously and brightly coloured clays and sands with lignite, all incoherent and highly inclined. Various opinions, however, have been put forth as to the relative age of the Martha's Vineyard strata, which were assigned by Prof. Hitchcock, at a time when the tertiary formations of the United States were less known, to the Eocene period, while Dr. Morton supposed them to be in part only tertiary, and that they rested on greensand of the cretaceous period.

The section at Gay Head is continuous for four-fifths of a mile, the beds dip to the north-east generally at an angle of from thirty-five to fifty degrees, though in some places at seventy degrees. The clays predominate over the sands. In one place Mr. Lyell found a great fold in the beds, in which the same osseous conglomerate and associated beds of white sand, on the whole fifty feet thick, were so bent as to have twice a north-easterly and once a south-westerly dip. In the yellowish and dark brown clay near the uppermost part of the section at Gay Head, and in the greensand immediately resting upon it†, Mr. Lyell found the teeth of a shark, that of a seal, vertebræ of

* Read Feb. 1, 1843. See Mr. Murchison's notice of the contents of this paper, p. 551 of our preceding volume.

† Nos. 5 and 6 of Prof. Hitchcock's section.

Cetacea, crustacean remains and casts of *Tellina* and *Mya*. These prevail at intervals through a thickness of nearly 100 feet, and are followed by beds of sand and clay with lignite. Mr. Lyell found no remains in the red clays. Many rolled bones were found in the osseous conglomerate.

In the section at Chilmark similar strata to those at Gay Head occur, but the general dip is south-west. Some of the folds, however, give anticlinal dips to the north-east as well as the south-west, and there are many irregularities, the beds being sometimes vertical and twisted in every direction. Several faults are seen and veins of ironsand, which intersect the strata like narrow dykes, as if there had been cracks filled from above. One bed of osseous conglomerate at Chilmark, four yards in thickness, is vertical, and its strike is well seen to be north 25° east, so that the disturbances have evidently been so great that it would be difficult without more sections to determine positively the prevailing strike of these beds. The incumbent drift is very variable in thickness, and large erratics, from twenty to thirty feet in diameter, are seen resting on quartzose sand. The author saw no grounds for concluding that any cretaceous strata occur anywhere in the island, nor could he find any fossils which appeared to have been washed out of a cretaceous formation into the tertiary strata, as some have suggested.

Mr. Lyell proceeds to the consideration of the organic remains collected by himself in Martha's Vineyard.

Mammalia.—1. A tooth, identified by Prof. Owen as the canine tooth of a seal, of which the crown is fractured. It seems nearly allied to the modern *Cystophora proboscidea*.

2. A skull of a walrus, differing from the skulls of the existing species (*Trichecus rosmarus*, Linn.), with which it was compared by Prof. Owen, in having only six molars and two tusks, whereas those of the recent have four molars on each side, besides occasionally a rudimentary one. The front tusk is rounder than that of the recent walrus.

3. Vertebræ of *Cetacea*, some of which are referred by Prof. Owen to the Whalebone-whales, and others to the Bottle-nosed (*Hyperoodon*).

Pisces.—Teeth of sharks resembling species from the Faluns of Touraine, viz. *Carcharias megalodon, Oxyrhina xiphodon, O. hastulis*, and *Lamna cuspidata*. With these were large teeth of two species of *Carcharias*, one resembling *C. productus*, a Maltese fossil. With the exception of the two last, Mr. Lyell found the same species in miocene strata near Evergreen, on the right bank of James River in Virginia.

Crustacea.—A species considered by Mr. Adam White as probably belonging to the genus *Cyclograpsus*, or the closely allied *Sesarma* of Say, and another, decidedly a *Gegarcinus*.

Mollusca.—1. Casts of a *Tellina* allied to *T. biplicata*, a miocene fossil, and of another near *T. lusoria*. 2. Cast of a *Cytherea* resembling *C. Sayana*, Conrad. 3. Three casts of a *Mya*, one of which bears a close resemblance to *Mya truncata*.

Mr. Lyell concludes, from the various evidence here given, that the strata of Martha's Vineyard are miocene. The numerous remains of Cetacea of the genera *Balæna* and *Hyperoodon* are adverse to the supposition of their being Eocene, while such fossils abound in the miocenc beds of America. The other fossils all point to a similar conclusion.

6. *Letter from* J. Hamilton Cooper, Esq., *to* Charles Lyell, Esq., V.P.G.S., *On Fossil bones found in digging the New Brunswick Canal in Georgia**.

Mr. Cooper prefaces his communication by a description of the country surrounding the locality in which the bones were found. The portion described is that part of the sea-coast of Georgia which lies between the Alatamaha and Turtle rivers in one direction, and the Atlantic Ocean and the head of tide water on the other. For twenty miles inland the land is low, averaging a height of from ten to twenty feet, and reaching, in some instances, forty feet, and consisting of swamps, salt-marshes, sandy lsnd, and clay loam. It then suddenly rises to the height of seventy feet, and runs back west at this elevation about twenty miles, at which point there is a similar elevation of between sixty and seventy feet. The whole of this district is a post-tertiary formation, and is composed of recent alluvium, and a well-characterized marine post-pliocene deposit. The recent alluvium is divided into inland-swamp, tide-swamp, and salt-marsh. The two last occupy a shallow basin having a depth of about twelve feet, the bottom and sides of which are the post-pliocene formation. This the author divides into three groups, in the last of which, constituting the elevated sand hills, no organic remains have been found; in the two former marine shells of existing species occur.

The fossil bones of the land mammalia discovered by Mr. Cooper, were found resting on the yellow sand and enveloped in the recent clay alluvium. Their unworn state and the grouping together of many bones of the same skeleton, render it highly probable that the carcasses of the animals falling or floating into a former lake or stream, sank to the sandy bottom, and were gradually covered to their present depth by the sedimentary deposits from the water. Among them were remains of the megatherium, *Mastodon giganteum*, mammoth, hippopotamus and horse. The fossil shells found in the post-pliocene, were species at present existing on the neighbouring shores.

The facts narrated by Mr. Cooper lead to the following conclusions :—1st. That the post-pliocene formation extends further south than Maryland, to which it has hitherto been limited. 2nd. The coexistence of the megatherium with the mammoth, mastodon, horse, bison, and hippopotamus. 3rd, That the surface of the country has undergone no sudden or violent change since those animals inhabited it, which is proved by the absence of all traces of diluvial action in the enveloping alluvium or surrounding country. 4th. That whatever changes of temperature may have taken place since that time, fatal to the existence of those mammalia, the identity of the fossil

* Read Feb. 1, 1843. See p. 552 of our preceding volume.

with the existing species of the marine shells of the coast shows that the temperature of the ocean at a period prior to the existence of the megatherium, the mastodon, and the hippopotamus was such as is congenial to the present marine testacea of Georgia.

7. *On the Geological position of the* Mastodon giganteum *and associated fossil remains at Bigbone Lick, Kentucky, and other localities in the United States and Canada.* By Charles Lyell, Esq., V.P.G.S*.

With a view to ascertain the relations of the soil in which the bones of the Mastodon are found, to the drift or boulder formation, whether any important geographical or geological changes had taken place since they were imbedded, and what species of shells are associated with them, Mr. Lyell visited a number of places where they had been obtained. In this paper he gives the result of his researches.

The most celebrated locality visited was Bigbone Lick, in the northern part of Kentucky, distant about 25 miles to the S.W. of Cincinnati, situated on a small tributary of the river Ohio called Bigbone Creek, which winds for about 7 miles below the Lick before joining the Ohio. A "Lick" is a place where saline springs break out, generally among marshes and bogs, to which deer, buffaloes, and other wild animals resort to drink the brackish water and lick the salt in summer. The country around Bigbone Lick, and for a considerable distance on both banks of the Ohio, above and below it, is composed of blue argillaceous limestone and marl, constituting one of the oldest members of the transition or Silurian system. The strata are nearly horizontal and form flat table-lands intersected by numerous valleys in which alluvial gravel and silt occur; but there is no covering of drift in this region. The drift is abundant in the northern parts of Ohio and Indiana, but disappears almost entirely before we reach the Ohio.

Until lately herds of buffaloes were in the habit of frequenting the springs, and the paths made by them are still to be seen. Numbers of these animals have been mired in the bogs, and horses and cows have perished in like manner. Along with their remains are found innumerable bones of Mastodon, Elephant, and other extinct quadrupeds, which must have visited these springs when the valley was in its present geographical condition in almost every particular, and which must have been mired in them as existing quadrupeds are at present. The mastodon remains are most numerous and belong to individuals of all ages. The mud is very deep, black, and soft. In places it is seen to rest upon the limestone, and at some points it swells up to the height of several feet above the general level of the plain and of the river. It is occasionally covered by a deposit of yellow clay or loam, resembling the silt of the Ohio, which is from 10 to 20 feet thick, rising to that height above the creek and often terminating abruptly at its edges. This loam has all the appearance of having been deposited tranquilly on the surface of the morass and of having afterwards suffered denudation.

* Read Feb. 1, 1843. See p. 552 of our preceding volume.

The Mastodon and other quadrupeds have been mired before the deposition of the incumbent silt, for a considerable number of fossil bones have been found by digging through it. Accompanying the bones are freshwater and land shells, most of which have been identified by Mr. Anthony with species now existing in the same region.

Mr. Lyell observes that the surface of the bog is extremely uneven, and accounts for it partly by the unequal distribution of the incumbent alluvium, which presses with a heavy weight on certain parts of the morass, from which other portions of the surface are entirely free. He also attributes it in part to the swelling of the bog where it is fully saturated with water near the springs.

The author is of opinion that the fossil remains of Bigbone Lick are much more modern than the deposition of the drift, which is not present in this district. But although the date of the imbedding of these mammalian fossil remains is so extremely modern, considered geologically, it is impossible to say how many thousand years may not have elapsed since the Mastodon and other lost species became extinct. They have been found at the depth of several feet from the surface, but we have no data for estimating the rate at which the boggy ground has increased in height, nor do we know how often during floods its upper portion has been swept away.

Ohio.—The Ohio river immediately above and below Cincinnati is bounded on its right bank by two terraces consisting of sand, gravel and loam, the lower terrace consisting of beds supposed to be much newer than those of the upper. In the gravelly beds of the higher terrace teeth both of the Mastodon and elephant have been met with. Mr. Lyell was assured that a boulder of gneiss, 12 feet in diameter, was found resting on the upper terrace, about 4 miles north of Cincinnati, and that some fragments of granite had been found in a similar situation at Cincinnati itself. These facts show that some large erratics have taken up their present position since the older alluvium of the Ohio valley was deposited. In travelling northwards from Cincinnati towards Cleveland, Mr. Lyell found the northern drift commence in partial patches 25 miles from the former city and about 5 miles N.E. of Lebanon, after which it continually increased in thickness as he proceeded towards Lake Erie.

New York—Niagara Falls.—In a former paper Mr. Lyell alluded to the position of the remains of Mastodon, 12 feet deep, in a freshwater formation on the right bank of the river Niagara at the Falls*. He remarks that if we had not been able to prove that the cataract had receded nearly four miles since the origin of the fluviatile strata in question, we should have been unable to assign any considerable duration of time as having intervened between the inhumation of the Mastodon in marl full of existing shells and the present period. The general covering of drift between Lakes Erie and Ontario is considered to be of much higher antiquity than the gravel containing the bones of the Mastodon at the Falls.

Rochester.—In the suburbs of this city remains of the *Mastodon*

* [See Phil. Mag. S. 3. vol. xxi. p. 554.]

giganteum were found associated with existing species of Mollusca in gravel and marl below peat.

Genesee.—Here remains of the *Mastodon giganteum* were found with existing shells in a small swamp in a cavity of the boulder formation, so that the animal must have sunk after the period of the drift when a shallow pond fed by springs was inhabited by the same species of freshwater mollusca as now live on the spot.

Albany and Greene Counties.—Mr. Lyell examined, in company with Mr. Hall, two swamps west of the Hudson River, where the remains of Mastodon occurred in both places at a depth of four or five feet, precisely in such situations as would yield shell marl, and peat, with remains of existing animals in Scotland. Cattle have recently been mired in these swamps.

According to Mr. Hall the greatest elevation at which Mastodon bones have been found in the United States is at the town of Hinsdale, situated on a tributary of the river Allegany in Cattaraugus county in the State of New York, where they occur at an elevation of 1500 feet above the level of the sea.

Maryland.—In the museum at Baltimore, Mr. Lyell was shown the grinder of a Mastodon, distinct from *M. giganteum*, and which had been recognised and labelled by Mr. Charlesworth as *M. longirostris*, Kaup. It was found at the depth of 15 feet from the surface in a bed of marl near Greensburgh, in Carolina County, Maryland, and is considered by Mr. Lyell as a miocene fossil.

Atlantic border.—Between the Appalachian mountains and the Atlantic there is a wide extent of nearly horizontal tertiary strata, which at the base of the mountains are 500 feet and upwards in height, but decline in level nearer the ocean and at length give place to sandy plains and low islands skirting the coast, in which strata containing marine shells of recent species are met with, slightly elevated above the sea. Occasionally deposits formed in freshwater swamps occur, below the mean level of the Atlantic or overflowed at high tide. In this district Mr. Nuttall discovered, on the Neuse 15 miles below Newburn, in South Carolina, a large assemblage of mammalian bones, including those of the *Mastodon giganteum*, resting on a deposit containing marine shells of recent species. Mr. Conrad presented Mr. Lyell with the tooth of a horse covered with barnacles, from this locality. Professor Owen has examined it and could find no corresponding tooth of a recent species, but considers it as agreeing with the horse-tooth brought by Mr. Darwin from the north side of the Plata in Entre Rios in South America.

South Carolina.—Remains of the Mastodon were found in digging the Santee Canal, in a spot where large quadrupeds might now sink into the soft boggy ground.

Georgia.—Bones of the Mastodon and Megatherium occur in this district in swamps formed upon a marine sand containing shells of species now inhabiting the neighbouring sea*.

Mr. Lyell in conclusion offers the following observations :—

1. That the extinct animals of Bigbone Lick and those of the At-

* *Antè*, p. 189.

lantic border in the Carolinas and in Georgia belong to the same group, the identical species of Mastodon and elephant being in both cases associated with the horse, and while we have the Mylodon and Megatherium in Georgia, the Megalonyx is stated by several authors to have been found at Bigbone Lick*.

2. On both sides of the Appalachian chain, the fossil shells, whether land or freshwater, accompanying the bones of Mastodons, agree with species of Mollusca now inhabiting the same regions.

3. Under similar circumstances Mr. Darwin found the Mastodon and horse in Entre Rios, near the Plata, and the Megatherium, Megalonyx and Mylodon, together with the horse, in Bahia Blanca in Patagonia; these South American remains being shown by their geological position to be of later date than certain marine Newer Pliocene, and Post-pliocene strata. Mr. Darwin also ascertained that some extinct animals of the same group are more modern in Patagonia than the drift with erratics.

4. The extinct quadrupeds before alluded to in the United States lived after the deposition of the northern drift, and consequently the coldness of climate which probably coincided in date with the transportation of the drift, was not as some pretend the cause of their extinction.

[* One of the conclusions to which the facts narrated by Mr. J. Hamilton Cooper, in his paper (*antè*, p. 189) on fossil bones found in Georgia, lead, is "the co-existence of the megatherium with the mammoth, mastodon, horse, bison, and hippopotamus." Mr. Lyell states, above, the co-existence of the elephant (mammoth) and mastodon with the horse in the Bigbone Lick and in the Carolinas; and also, on the authority of Mr. Darwin, that of the mastodon and horse near the Plata, and of the megatherium, megalonyx, mylodon, and horse in Patagonia. A parallel case, to a certain extent, is afforded in the extreme north of the American continent, by the association in Eschscholtz Bay, of bones of the elephant (mammoth), bison (urus), musk-ox, deer, and horse; and if the writer of this note be correct in assigning to the megatherium a cervical vertebra, hitherto unappropriated, in the collection from Eschscholtz Bay, the parallelism of the case there presented with that occurring in Georgia will be very close; since in both localities we shall then have the co-existence of the megatherium, mammoth, horse, and bison. And further, the megatherium will then appear to have extended from the extreme south (Patagonia) to the extreme north (Eschscholtz Bay) of the New World; and to have been associated, throughout its range, with the horse, if not indeed with the other mammalia here enumerated. The former association, in America, of mammalia almost universally distributed, at some geological period, over Asia and Europe, but the living analogues

of which are now confined to the Old World, with others, which not only appear to have been peculiar to America, but the living analogues of which are also now confined to that continent, forms a very interesting and important subject of investigation in palæontological geography. See Dr. Buckland's memoir "On the occurrence of the remains of Elephants," &c. "in Eschscholtz Bay," forming part of the Appendix to Capt. F. W. Beechey's "Narrative of a Voyage to the Pacific and Beering's Strait performed in H.M.S. Blossom in the years 1825, 1826, 1827, 1828," pp. 593, 597; and a paper supplementary to Dr. Buckland's memoir, by E. W. Brayley, Jun., on the "*Organic Remains in the Diluvium of the Arctic Circle, and on the probability that one of the Fossil Bones brought from Eschscholtz Bay belonged to a species of Megatherium*," inserted in Phil. Mag. S. 2. vol. ix. pp. 411, 416. The vertebra in question, it appears from Dr. Buckland's memoir, was included in the series of specimens selected from the Eschscholtz Bay fossils for the British Museum. It may tend to abbreviate the labours of future inquirers on the subject, to add that it is not alluded to, either in Mr. Clift's account of the remains of the Megatherium sent from Buenos Ayres by Sir Woodbine Parish, Trans. Geol. Soc. S. 2. vol. iii. p. 437; or in Professor Owen's elaborate work on the *Mylodon robustus* and Megatherioid Quadrupeds in general. Nor is it mentioned by Dr. Buckland in his Bridgewater Treatise, although he notices (vol. i. p. 142 *note*) the apparent extension of the Megatherium "north of the equator as far as the United States," instancing some former observations of the occurrence of its bones and teeth in Georgia.—E. W. B.]

XXV. *Account of a Hydro-electric Machine constructed for the Polytechnic Institution, and of some Experiments performed by its means.* By W. G. Armstrong, *Esq.* *

To Michael Faraday, Esq., &c. &c. &c.

Dear Sir,

The following account of an electric boiler, which has been recently constructed under my superintendence, and of certain experiments which have been made with it, is addressed to you, not only because you have lately investigated with your usual ability and success the subject of steam electricity, but because the results of the experiments which I am about to describe are calculated to elucidate and establish some of your views respecting the nature and identity of the various species of electricity.

* Communicated by Dr. Faraday, an abstract of whose paper alluded to by the Author will be found in Phil. Mag. S. 3. vol. xx. p. 486.

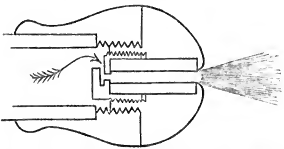
The powerful effects which I obtained in the autumn of last year, with the electric boiler which I then used, induced me to offer to procure one to be made, of a large size, and upon an improved construction, for the Polytechnic Institution in London. My offer was accepted, and the apparatus, which, in conformity with your theory, I shall in future call a "Hydro-electric Machine," has recently been completed, and will shortly appear at the Institution for which it is destined; where I hope, with proper arrangements for carrying off the discharged steam, it will act as well as it has done in the open air.

I shall now endeavour to give you a general idea of the nature of the apparatus, and will then proceed to speak of its effects.

The boiler is a cylinder made of rolled iron plate, and measures three feet six inches in diameter, and six feet six inches in length, exclusive of the smoke chamber, which forms an extension of the cylinder, and makes the extreme length seven feet six inches. The fire-place is contained within the boiler, and the heated air is conveyed through the water, in tubular flues, to the smoke chamber, to which a chimney is attached. The apparatus is supported, at a height of three feet from the ground, upon six strong pillars of dark green glass, which insulate it very effectually; and the steam is discharged from forty-six jets, to each of which it is conveyed through an iron condensing pipe, in which the cold of the external air causes the deposition of the proper proportion of water to be ejected with the steam.

Fig. 1 represents one of the jets. It consists of a brass

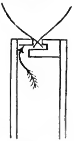
Fig. 1.



socket containing a cylindrical piece of partridge wood, with a circular hole or passage through it one-eighth of an inch in diameter, into which the steam is admitted through an aperture, similar to that which I have minutely described in the Philosophical Magazine of January last. [S. 3. vol. xxii. p. 1.]

The peculiar shape of this aperture appears to derive its efficacy from the tendency it gives the steam to spread out in the form of a cup on entering the wooden pipe; and by that means to bring it, and the particles of water of which it is the carrier, into very forcible collision with the rubbing surface of the wood. This explanation is not mere conjecture, for I find that when water is forced with a strong pressure through a similar aperture, it is dished out in the manner shown in fig. 2.

Fig. 2.



The steam is discharged against a range of metallic points communicating with the ground, by which its electricity is carried off and so prevented from retroceding to the boiler. These points are placed very near to the jets, in experiments which require a large quantity of electricity, without great length of spark; but when high tension is an object, they are removed to a distance of three or four feet from the discharging apertures.

As an example of the power of this machine in charging jars, I may state that my friend Captain Ibbetson, one of the Directors of the Polytechnic Institution, who lately visited me for the purpose of seeing the machine, and who has co-operated with me in most of the experiments made with it, brought with him from London a large Leyden jar, which had discharged spontaneously fifty times in a minute when tried with the colossal plate machine belonging to the Institution; and that when this jar was applied to the boiler, it gave 140 similar discharges in the like space of time.

The spark which the boiler produces, although occasionally reaching twenty-two inches in length, is by no means commensurate with its other effects.

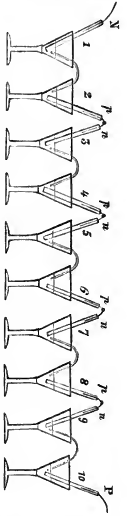
Its greatest power is manifested when the electricity is drawn off merely as a current, without any disruptive discharge; and the results I have obtained, when using it in this way, will, I conceive, prove highly interesting to you.

The true polar electro-chemical decomposition of water, which has never hitherto been unequivocally performed by frictional electricity, has been effected in the clearest and most decisive manner by means of this machine; and I shall now describe an experiment in which this interesting effect was combined with other curious phænomena.

Ten small wine-glasses were arranged as shown in fig. 3, and into each glass was poured an equal measure of the liquid named opposite to the glass containing it. A glass tube, closed at one end around a platina wire, which extended an inch and a quarter into the tube, was then inverted in each

glass after having been filled with a portion of the liquid contained in it. The tubes were all of the same size, being three

Fig. 3.



Distilled water ....

Distilled water ....

Distilled water acidified with one-sixth of its volume of sulphuric acid ....

Distilled water acidified with one-sixth of its volume of sulphuric acid ....

Solution of sulphate of soda reddened with acidified litmus ....

Solution of sulphate of soda coloured blue with litmus ....

Solution of sulphate of magnesia reddened with acidified litmus............................

Solution of sulphate of magnesia coloured blue with litmus ....

Distilled water reddened with acidified litmus...

Distilled water coloured blue with litmus ......

inches and a half long and one-sixth of an inch wide in the inside. The platina wire of the first tube was connected with the boiler, and that of the last with a discharging train, consisting of a lead pipe which passed into a neighbouring well. The wires of the other tubes were connected in pairs, and wet cotton placed between every alternate glass, as shown in the

figure. Under these circumstances you will perceive that the tubes in the glasses numbered 1. 3. 5. 7. and 9 contained negative poles, and that the remaining tubes contained positive poles.

Upon setting the machine to work, a stream of small bubbles immediately began to rise from all the wires, and it soon became evident that the gas which collected in the tubes containing the negative poles, occupied exactly twice the volume of that which was evolved from the positive poles. In the course of two or three minutes the red liquid in number 9, which you will observe consisted of nothing but distilled water and acidified litmus, became blue around the wire in the tube; while the blue liquid in number 10, consisting only of water and blue litmus, was to the same extent changed to red. As the process continued a similar change began to take place in numbers 5 and 6, containing the solutions of sulphate of soda, and in numbers 7 and 8, containing the solutions of sulphate of magnesia; but the transition from blue to red, and from red to blue, was not nearly so rapid in these vessels as in 9 and 10, where no salt was present, to yield by decomposition an acid at the one pole, and an alkali at the other.

As soon as the pressure in the boiler had run down from 75lbs. to 40lbs. on the square inch, the steam was shut off until the original pressure was re-attained, when the machine was again put in action; and by repeating this operation several times, I obtained as much gas in each of the tubes containing the negative wires, as occupied nearly an inch from the top; and half that quantity by measure in each of the tubes containing the positive wires.

At the close of the experiment the change of colour from red to blue in number 9, and from blue to red in number 10, was perfect, and extended to the whole of the liquid in each of those glasses as well as in the tubes contained in them. In the other glasses, containing the solutions of sulphate of soda and sulphate of magnesia, the change of colour was also considerable, but not nearly so much so as in 9 and 10, although at the beginning of the experiment the quantity of colouring matter in all these glasses had been the same.

The proportions in which the gases were evolved from the two poles, sufficiently indicated them to be hydrogen in the one case, and oxygen in the other; and it is scarcely necessary to say that upon examination they proved to be such.

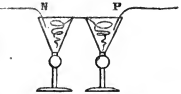
I could perceive no difference in the quantity of gas, of the same kind, which had collected in the different tubes, and the decomposition seemed to be neither accelerated nor retarded by making a small interruption in the conducting wire, so as

to cause the electricity to pass in short sparks, instead of a uniform current.

The collective periods during which the machine was in action, while accomplishing these effects, amounted to about an hour and a quarter, but by using very narrow tubes, and operating upon small quantities of liquid, I could obtain equally decisive results in the space of eight or ten minutes.

In making some experiments similar to the one I have just described, I perceived that when the electric current was passed through two glass vessels containing pure water, and communicating with each other by means of wet cotton, the water rose above its original level in the vessel containing the negative pole, and subsided below it in that which contained the positive pole, indicating the transmission of water in the direction of a current flowing from the positive to the negative wire. The investigation of this phænomenon led me to a most unexpected and remarkable result, which, if I mistake not, will greatly excite your interest.

Fig. 4.



Two wine-glasses, N and P, fig. 4, filled nearly to the edge with distilled water, and placed about four-tenths of an inch from each other, were connected together by a wet silk thread, of sufficient length to allow a portion of it to be coiled up in each glass as represented in the figure. The negative wire, or that which communicated with the boiler, was inserted in the glass N (which I shall call the negative glass), and the positive wire, or that which communicated with the ground, was placed in the glass P (which I shall call the positive glass). The machine being then put in action the following singular effects presented themselves.

1st. A slender column of water, inclosing the silk thread in its centre, was instantly formed between the two glasses, and the silk thread began to move from the negative towards the positive pole, and was quickly all drawn over and deposited in the positive glass.

2nd. The column of water after this continued for a few seconds suspended between the glasses as before, but without the support of the thread; and when it broke the electricity passed in sparks.

3rd. When one end of the silk thread was made fast in the negative glass, the water diminished in the positive glass, and increased in the negative one; showing apparently that the motion of the thread, when free to move, was in the reverse direction of the current of water.

4th. By scattering some particles of dust upon the surface of the water, I soon perceived by their motions that there were two opposite currents passing between the glasses, which, judging from the action upon the silk thread in the centre of the column, as well as from other less striking indications, I concluded to be *concentric*, the inner one flowing from negative to positive, and the outer one from positive to negative. Sometimes the outer current, or that which I assumed to be such, was not carried over into the negative glass, but trickled down the outside of the positive one; and then the water, instead of accumulating as before in the negative glass, diminished both in it and the positive glass.

5th. After many unsuccessful attempts, I succeeded in causing the water to pass between the glasses, without the intervention of the thread, for a period of several minutes; at the end of which time I could not perceive that any material variation had taken place in the quantity of water contained in either glass. It appeared therefore that the two currents were nearly, if not exactly equal, when the inner one was not retarded by the friction of the thread.

As you are so much more competent than I am to draw conclusions from this curious experiment, I shall not advance any opinion upon the subject, further than by observing that it appears to me to be eminently calculated to elucidate the nature of the electric current.

It is proper to state that I found it essential to the success of this experiment, that the water in the glasses should be perfectly pure. The slightest contamination caused the water to boil upon the thread, instead of passing between the glasses in the manner I have described, and the instant the thread became nearly dry, it was destroyed by the heat elicited by the current of electricity. To ensure success it was necessary to use water distilled in glass vessels, for I found that the common distilled water which I obtained at the chemists' shops, was in general insufficiently pure for the purpose.

Amongst various other cases of electro-chemical action effected by this machine, I may instance the coating of a small silver coin with copper, by attaching it to a platina wire which formed the negative pole in a solution of sulphate of copper, but a long-continued action of the machine was required before this was accomplished. I may also mention the decomposition of iodide of potassium to such an extent as to colour a deep blue wine-glass-full of the solution in a very short time, when starch and a few drops of hydrochloric acid were present. When the hydrochloric acid was omitted, the mixture in general changed to an amber instead of a blue colour.

A magnetic needle, suspended by a fibre of silk between

the convolutions of a multiplying wire which made sixteen folds, and having its terrestrial magnetism partially neutralized by a second needle in the usual way, was immediately deflected by passing the current through the wire, and retained in a state of oscillation between angles of about 20° and 30°. On reversing the current the deflection took place in the opposite direction, precisely as it would have done if voltaic electricity had been used.

A cylinder of soft iron, nine inches long and one inch in diameter, wrapped with about eighty feet of copper wire, covered with cotton and thickly varnished, had sufficient magnetism excited in it to influence very sensibly a compass needle placed in its vicinity.

Fig. 5.

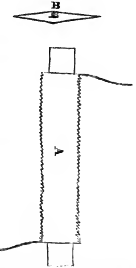


Fig. 5 will illustrate the manner in which this experiment was performed. A is the bar of soft iron with the copper wire coiled upon it, and B the compass needle which turned upon a point and was placed with one of its poles at a distance of two inches from the nearest extremity of the iron bar. Upon passing the current of electricity through the wire, the needle moved 5° towards the bar and returned to its original place when the current ceased. Again, when the direction of the current was reversed, the needle was repelled about 3½°, making a total variation between the two extremes of about 8½°.

The time during which the machine remained in my hands after its completion did not enable me to follow up these various experiments to the extent I should have wished, and I must therefore leave to others the task of prosecuting them further.

Before I conclude this letter, I must beg to disclaim the opinion which has been attributed to me in some notices of your recent lecture upon Steam Electricity, viz. that the electricity arose from the passage of the water into the aëriform state. I have long held that the emission of a certain proportion of water in conjunction with the steam, was essential to a high development of electricity; and also that the effect depended in a very great measure upon the nature and form of the discharging orifice; and it has been by acting upon these principles, for more than a year, that I have been enabled

to bring my apparatus to its present degree of efficiency. My opinions upon these points will be found recorded in two papers which appeared in the Philosophical Magazine, one in January 1842, [S. 3. vol. xx. p. 5.] and the other in the same month of the present year; but in both these papers I expressed a doubt as to *friction* being the *exclusive* cause of the excitation. Some of the difficulties which I then felt in ascribing the effect to friction have been cleared away by your masterly investigation of the subject; but others I am bound to say still remain unshaken. In operating with a small boiler made of bronze or gun-metal, I have seen the apparatus pass from one electrical state to the other, under circumstances which appeared to preclude the possibility of any change having taken place in the condition either of the steam or the watery particles ejected with it. I have also tried numerous variations of the discharging orifice, which proved much less effective than a simple cylindrical passage, although apparently calculated to produce a far greater degree of friction. Still however I believe that the effects are essentially owing to friction, either of the steam and water combined, or of the water alone; and I think it probable that all the phænomena which have been observed will ultimately be reconciled with the theory which you have so boldly and explicitly announced.

I remain, dear Sir, yours very truly,

Newcastle-upon-Tyne, August 12, 1843. W. G. Armstrong.

XXVI. *Letter from* Dr. Hare *on* Professor Daniell's *Defence of the view taken by the latter of certain Electrolytic experiments, which have been represented as proving the existence of a compound radical (oxysulphion) in certain sulphates.*

To R. Phillips, Esq.

Dear Sir, Philadelphia, June 30, 1843.

1. I HAVE read in the Philosophical Magazine for June, [S. 3. vol. xxii. p. 461.] a friendly letter to you from Professor Daniell, in reply to some strictures made by me upon one of the arguments advanced in proof of the existence of compound radicals in certain salts. Hoping that you will honour the remarks which I am about to make with a place in the same work, and presuming that no reader will favour them with a perusal who has not read or cannot refer to the letter in question, I will proceed as if that letter were before us.

2. I had advanced, that when aqueous solutions of oxysalts, of which the base is a metallic oxide, the sulphates of soda,

potash, or copper for instance, are subjected to electrolysis, it is the base which is the direct subject of electrolyzation, the liberation of acid and evolution of hydrogen being secondary results. Objecting to this opinion, the distinguished author of the letter alleges "there is nothing, I think, that I should have anticipated less than that a solution of sulphate of soda would be affected by the voltaic current precisely in the same way as a solution of caustic soda, or that the powerful affinities of the acid and the base should have no influence upon the result."

In reply to this objection, I beg leave to point out, that so far is the *direct* influence of chemical affinity from being hostile to electrolysis, that the more energetic the attraction between the elements of an electrolyte, the more is it susceptible of electro-chemical decomposition. According to Faraday, "from the period when electro-chemical decomposition was first effected to the present time, it has been a remark that those elements which in the ordinary phænomena of chemistry were most directly opposed to each other and combine with the greatest attractive force, were those which were most readily evolved at the opposite extremities of the decomposing bodies." (See Researches, page 198, paragraph 669.) But if electrolysis be not impeded by the *direct* resistance of the chemical affinity, is it consistent that it should be controlled by an *indirect* resistance of the same force, such as is exercised between the acid and the soda in the case under consideration?

3. Professor Daniell, in support of the view of the subject which he has taken, alleges, that "it will be easy for Dr. Hare to convince himself by a few easy experiments that a solution of sulphate of soda submitted to electrolysis becomes acid in the zincode division of a diaphragm cell, not only by the *abstraction of sodium* from it, but by the *accumulation of acid* transferred from the platinode division; just as he will find an accumulation of soda in the latter arising from the secondary action of the sodium transferred from the former."

The ingenious author of these allegations has, however, omitted to show that, supposing them to be true, they will be more consistent with the idea, that the electrolyte which undergoes decomposition is an oxysulphionide than an oxide. Is it not clear that, according to either conception, there can be no more, nor no less acid and alkali isolated, than one equivalent of each for every atom of oxygen evolved at the anode?

4. Agreeably to the idea that oxysulphionide of sodium is the subject of electrolysis, by a series of decompositions and

recompositions, an atom of oxysulphion is liberated at the anode, while an atom of sodium is liberated at the cathode. These severally, by acting on water, are converted into soda and oxysulphionide of hydrogen, evolving at the cathode one atom of hydrogen for each atom of soda, and one atom of oxygen at the anode for each atom of oxysulphionide of hydrogen, or in other words, for each atom of free sulphuric acid.

5. Agreeably to the idea that oxide of sodium is the electrolyte, for each atom of water decomposed by the sodium evolved at the cathode, there will be an atom of hydrogen evolved, and an atom of soda generated, while for each atom of oxygen liberated at the anode by the decomposition of the soda, in which it existed, there will be an atom of acid set free, or according to the nomenclature of Professor Daniell, left in the state of oxysulphionide of hydrogen. Hence consistently with either view of the phænomena, the transfer of acid and soda can only proceed *pari passu*, atom for atom, with the evolution of oxygen and hydrogen at the anode and cathode respectively.

6. According to Professor Daniell, "it is my oversight of the fact that the acid accompanies the oxygen to the zincode while the metal travels to the platinode, that causes his experiments with the membrane to appear to me complicated and confused." It was stated in my pamphlet, that when by means of a membrane dividing the space between electrodes, two fluids were subjected to the voltaic current, as for instance a solution of potash and sulphate of copper, there would be, according to one way of viewing the subject, a continuous row of oxygen anions from the cathode to the anode; the cathions being on one side of the membrane potassium, on the other copper. According to the view preferred by Daniell, the anions on one side would be oxygen, on the other oxysulphion, but evidently oxysulphion could not pass by electrolytic decomposition and recomposition beyond the point at which it is replaced by oxygen in the row of anions. Of course it is as inconsistent with one view as the other, that free acid "oxysulphionide of hydrogen" should be liberated within the zincode cell when not containing a sulphate.

7. No effort is made by the distinguished Professor to explain how the copper can, as alleged by him, yield up its charge of electricity to hydrogen, without uniting with the oxygen of the water, in which that hydrogen existed.

8. The allegation that the precipitation of the copper ensues because "that metal finds nothing by combining with which it can complete its course," seems to be coupled with

error; first, because in a chemical point of view, a metal cannot displace the hydrogen of water without uniting with the oxygen; and secondly, because no electrolysis can originate without a simultaneous action ensuing in all the anions and cathions forming the electrolytic row, subjected to the decompositions and recompositions essential to that process. Had there been no anion to combine with the cupreous cathion at the membrane, how could the electrolysis have taken place? Agreeably to my apprehension, as well might it be represented, that a chain should continue to suspend a weight after any of its intervening links should be severed, as that electrolysis should proceed throughout any row of electrolytic atoms, when in any portion of the row there should be a cathion having no corresponding anion to combine with.

9. I call on Professor Daniell for replies to these queries. In yielding its electricity to the hydrogen of water, how could the copper be at a loss for something to combine with to complete its course, when the oxygen of that water was necessarily present?

10. If the copper cathion had no anion to combine with, how could it discharge itself upon the hydrogen?

11. If in the row of electrolytic atoms in which the cupreous cathions were situated, there was a point where there was no anion, how could the electrolysis have commenced?

12. Professor Daniell states that it is to him unintelligible, that a solution of potash on one side of a membrane and a solution of sulphate of copper on the other, can act as electrodes while subjected to electrolysis. But no reason is assigned why it is more unintelligible that these solutions should have acted in the capacity in question, than that two strata, one consisting of sulphate of magnesia, the other of pure water, should have served as electrodes in an experiment of Faraday, to which I referred, and which he has himself cited in his Chemical Philosophy with all the deference due to the well-known accuracy of the distinguished author. But I beg leave to inquire whether each of the solutions above-mentioned is not a conductor of electricity, whether any two conductors are not competent to act as electrodes, and whether a simultaneous exposure to electrolysation would deprive them of that competency?

I remain with esteem, your friend,

Robert Hare.

P.S. Allow me to correct an error copied into the Philosophical Magazine from my pamphlet. S. 3. vol. xxii. p. 464, paragraph 81, line 4, *for* cathion *read* anion.

XXVII. *On the Storms of Tropical Latitudes.* By William Brown, *Jun.* *

THE intention of this paper is to show that the explanation given in my essay on the "Oscillations of the Barometer," inserted in this Magazine of June 1842, of the descent of the barometer during the south-west storms of high latitudes, and its subsequent ascent as the storm changes to north, may be extended to the phænomena presented by the hurricanes of tropical latitudes, and that it may be generalized as follows:—All winds, of whatever force, which depress the barometer in any considerable degree, are caused by the descent of the upper current of the atmosphere. The velocity or momentum which this current has acquired in flowing from higher columns of the atmosphere to lower, causes it, instead of changing its course to that of the lower one, to flow along the surface of the earth in its original direction, in the place of the opposite or lower current of heavy air, which is necessary to maintain the ordinary pressure of the atmosphere. A diminution or rarefaction of the air is consequently produced, which increases until the force of the current is spent or is unequal to the opposite force, that of greater density, which then impels the air to flow towards the rarefied atmosphere occupying the locality of the storm, and restores it to its former density and pressure†.

These forces act in the direction of the meridian, but the force and direction of the wind are compounded of these forces and those produced by the rotation of the earth.

Further, a rarefaction of the air produced at any station A by the flow of the air towards another B, causes fresh portions of the upper current to descend behind A, and produces a constant recession of the storm, which, when modified by the rotation of the earth, occasions the progressive movement of hurricanes.

As these views, however, are opposed to opinions already advanced on this subject, it will be necessary before proceeding further to examine their foundation.

* Communicated by the Author.

† In the calculations then given in illustration, for the sake of obtaining data for the calculation of the extent of the depression of the barometer to which it might be reduced by this cause, I have limited the depression to the time when the actual height of the atmospheric column, or in other words the elasticity of its uppermost portions, is reduced to that of the colder columns towards which the upper current is flowing. It is obvious, however, that the only limit to the diminution of pressure is the destruction of the momentum of this current by the resistance opposed to it; and this, as is evidently the case, between the tropics and probably in high latitudes also, may not take place until the height of the column has been reduced much below this.

We are principally indebted for our knowledge of the phænomena of storms to three observers, W. C. Redfield and J. P. Espy of the United States*, and Colonel Reid, whose labours, whether at present successful or not in leading to a full explanation of their action, must ever be regarded with an interest, independent of that which attaches to them merely as important acquisitions to science, because of their being undertaken at the instigation of benevolence, in order if possible to mitigate the sufferings of mariners, by enabling them to escape sooner than they otherwise would from the fury of those terrific scourges of tropical seas.

J. P. Espy has laboured to show that the direction of the wind during storms is from all parts of their locality towards a central space or line; for which he accounts by supposing that the air within this space is so expanded by the latent heat emitted by the condensation of vapour, that a rush of air is occasioned from all sides towards it; a supposition obviously contradicted by the indications of the barometer, although it will be seen (fig. 2, page 214) that the directions of the wind observed by him agree with the views here set forth. To W. C. Redfield we owe the discovery of the progressive movement of hurricanes, and the revival of the hypothesis that storms are whirlwinds; an opinion which has been supported by Colonel Reid, and which explains so beautifully many of the phænomena, that it has met with a very favourable reception from many philosophers in this country: nothing however has yet been adduced, as I trust to be able to show, to prove its truth.

The phænomena on which this theory is founded are, the various directions of the wind in different portions of the storm, its great velocity compared with the rate of progress of the storm, and the veering of the wind, which generally changes in the northern hemisphere, in the same direction as that of the movement of the hands of a watch on the right of the storm (following the line of its progress), and in the contrary direction on the left,—all which facts would result from a progressing whirlwind revolving in the contrary direction to that of the hands of a watch. In the southern hemisphere the order of the veering of the wind is reversed.

But it appears to be principally on the two latter points that Redfield rests for the support of his theory; he says, in his essay on the "Hurricanes of the Atlantic" (American Journal of Science, vol. xxxi. p. 122), "The veering of the wind which so often occurs, when duly considered, is in itself a com-

* See p. 92 of our last Number.—Edit.

plete demonstration of the fact in question." And in the same essay (p. 125) he says, in speaking of the storm of August 1830, "It occupied about seven days in its ascertained course from near the windward islands, a distance of more than 3000 miles, the rate of its progress being equal to eighteen miles an hour. If we suppose the actual velocity of the wind in its rotatory movement to be five times greater than this rate of progress, which is not beyond the known velocity of such winds, it will be found equal in this period to a rectilinear course of 15,000 miles. The same remark applies in substance to all the storms which are passing under our review. What stronger evidence of the rotative action can be required than is afforded by this single consideration?"

Now in the passages here quoted, after the existence of a whirlwind has been assumed to account for certain phænomena, each individual explanation it is capable of giving is made a demonstration of its truth, without regard to the accordance between its further requirings and the facts found by observation. Let us then try it by some very simple results deducible from it. The velocity of the air in a progressing whirlwind must be very different on its opposite sides. If its greatest velocity be ninety miles an hour, and the hurricane progresses at the rate of eighteen, the velocity of the wind on the side where it is opposed to the progressive motion will be thirty-six miles less, or fifty-four miles an hour.

Hence the path of such a hurricane would be marked by two distinct sides, the difference of velocity being far too great not to afford a very decided characteristic; and as the force of the wind gradually lessens and the rate of progress increases as the storm advances, the storm on one side, during the latter part of its path, would be reduced to at least a moderate breeze. No such result, however, appears from observation; for although the same storm varies at different localities in force, these variations have no relation to the direction of the progressive movement.

Again, the fall of the barometer during the first part of the hurricane, and its rise on the change of the wind to the opposite quarter, are accounted for by supposing the air to be carried by the centrifugal force of the whirlwind from the centre towards the circumference.

A whirlwind can only be conceived to be maintained in motion in three ways,—1st, by an ascending column of air in the centre; 2nd, by the deflection of a rectilinear current by the resistance of the air on the outside of the whirl; 3rdly, by the deflection produced by a force directed to the axis of the whirlwind. In the first of these the explanation of the variation in

the height of the barometer would not be applicable; the second is evidently unequal to the effect; the last therefore is the only one in which a whirlwind, such as that assumed, can be supposed to be maintained. Now if the air moved in a perfect whirl, it is evident that no alteration of pressure in any part of the atmosphere within it would take place, the air would revolve at a constant distance from the centre; but this is not the case in the present instance; the air is not brought back on its revolution to exactly the place it previously occupied, but to one a little more remote from the axis; because it is found that storms enlarge in diameter as they advance: now the only rarefaction which the centrifugal force can effect, is that occasioned by the air thus increasing its distance from the axis, and therefore it is limited by the amount of the increase of the diameter of the storm. Now let the whole path of a hurricane be 3000 miles, and let its diameter on attaining the end of it be doubled, then a rate of progress of eighteen miles an hour will give only an increase of $\frac{1}{168}$th of the diameter in one hour; this enlargement therefore produces the whole amount of the diminution of the air within the whirlwind (whose height is not supposed to extend very far above the earth), which is required to be restored by the flow of the air of the higher strata of the atmosphere into the rarefied portion, an amount so small that this flow must be almost adequate to its entire restoration, so that only a decrease in the pressure of the air, almost trifling compared with what actually occurs, could take place from this cause.

But it is evident that the only direct proof of a rotative action must result from the accordance between the directions of the wind required by the hypothesis and those found by observation. But all the observations so laboriously collected by J. P. Espy, go to show that the direction of the wind is totally irreconcileable with the existence of a gyratory motion; and although they may be viewed with some suspicion on account of their being advanced in defence of a favourite theory, his results are too consistent with each other to be set aside. But independently of these, the facts given in Colonel Reid's volume afford results, of a different kind from those obtained by Espy, equally opposed to this theory.

If the direction of the wind were taken at an equal number of stations upon radii of a whirlwind equally distant from each other, the numbers representing the directions of the wind for the several points of the compass, making allowance for the progressive motion, would be equal. Now it is obviously impossible that this test could be rigorously applied to any hurricane; but when a great number of observations

have been obtained from a large portion of the track of a storm, during which numerous revolutions of the wind would be made, there ought to be some approach to equality. Colonel Reid has inserted in his work charts of two storms, the data of which are extracted from Redfield's writings; and hence we may suppose them at least as well adapted as any others to illustrate the opinions he is supporting. Below are given the numbers of each wind for sixteen points of the compass (the directions being taken from the data themselves); their striking discordance with the equality required will be seen, and also that it is very little diminished by adding the results of each storm together, showing that there are certain points of the compass almost entirely wanting to complete the circuit.

	N.	NNE.	NE.	ENE.	E.	ESE.	SE.	SSE.	S.	SSW.	SW.	WSW.	W.	WNW.	NW.	NNW.
Chart 1st.	0	2	5	1	2	5	8	2	0	0	1	1	4	2	5	1
Chart 2nd.	2	4	7	3	3	0	4	3	4	1	5	0	3	1	7	0
Sum ...	2	6	12	4	5	5	12	5	4	1	6	1	7	3	12	1

These storms were both moving from S.S.W. to N.N.E., hence the deficiency of the north and south points and those adjacent cannot result from the progressive motion.

W. C. Redfield has endeavoured to obtain evidence in favour of this theory from the tracks exhibited by vessels exposed to hurricanes; with what success will appear from the following instance. He says (American Journal of Science, vol. xxxi. p. 123), "It can but seldom happen however that the track of a vessel which scuds through a gale will fully develope the entire circle of the wind, the combination of circumstances necessary to this result being but rarely encountered; still I have obtained notice of a few such cases. A respectable ship-master not long since informed me, that he once scudded for twenty-four hours under a typhoon in the Chinese Sea, and on its departure found himself nearly in the position where he first took the gale."

Now supposing the progressive motion of the hurricane to have been at the rate of fifteen miles an hour, in twenty-four hours the storm would have moved 360 miles, a space the utmost which has been assigned as the diameter of the storms of these latitudes; hence the vessel must have entered the storm precisely at one extremity of the diameter and emerged at the other, and must have moved once and a half round the whirl; a series of coincidences so fortuitous, that a fact requiring them to reconcile it with an hypothesis cannot surely be regarded as affording any evidence in favour of the hypothesis itself. The fact however is in perfect accordance with the action of two opposite and consecutive rectilinear currents.

If it be thought that the objections here brought forward are not founded on sufficient observations to amount to a refutation of this theory, it will at least be allowed, that viewing it in the most favourable light, it remains simply as an hypothesis, assumed to account for certain phænomena, towards proving the truth of which nothing has yet been done, though it is one which is from its nature capable of being tested by an appeal to observation; and therefore in the absence of proof we may ask, how such a whirlwind can be produced and maintained in action whilst advancing over a tract of the earth's surface of three or four thousand miles in length, by the action of the known causes of disturbance to the atmosphere? Whence arises the centripetal force which deflects the air from a rectilinear course? If indeed the existence of such a whirlwind could be established by direct proof, it would justify the language used by its supporter, "that the long-cherished theory which is founded upon calorific rarefaction, must give place to a more natural system of winds and storms" (American Journal of Science, vol. xxxv. p. 222), although in what way it can be "founded upon the more simple conditions of the great law of gravitation" exclusively of the effects of difference of temperature, is by no means easy to conceive.

Leaving here the consideration of this theory, I may proceed to the development of the proposition with which this paper commenced; premising, that as the phænomena of hurricanes in the two hemispheres have been shown by Col. Reid to correspond with each other, in relation to the position of the equator and the poles, the signs north and south are here used in reference to the northern hemisphere alone; by which their double signification when applied to meteorological phænomena is avoided, and they may be used synonymously with polar and equatorial. The language may of course in every instance be applied to the southern hemisphere by reversing these signs.

The atmospheric phænomena within the tropics are apparently so much more dependent on general causes than in other regions of the globe, that meteorologists have endeavoured to explain those of the latter, by endeavouring to discover in them some feature, which, though obscured by its association with others, may yet be sufficiently perceptible to characterize them as analogous to phænomena within the tropics, whose more isolated occurrence has enabled us to refer them to known causes. In the present investigation, however, the reverse is the case; the reason of which I think will readily appear from the following:—

The one grand cause of atmospheric changes is the varying

position of the sun; but in one very material circumstance the regions about the equator are subject to a variation to which temperate regions are not exposed; this is the alternate north and south declination of the sun, occasioning at some periods of the year a partial or entire reversal of the regular currents of the atmosphere, and thus producing the apparent disparity between the storms of temperate and tropical latitudes.

The facts which may be considered as established regarding hurricanes within the tropics are the following: the wind at the onset of the storm usually blows from north-east, generally more easterly than the trade-wind, and sometimes from north or north-west. After blowing some time a calm frequently ensues, after which the wind again blows as violently as before, but from south-east or south-west. On the east side of the storm the wind veers from north to south (sometimes without the intervention of a calm) by east, but on the western side frequently if not always by west. The barometer falls during the first part of the storm, and rises again with the second part. Hurricanes commencing about the 10th or 15th degrees of latitude, advance in a curve somewhat of a parabolic form, from east-south-east to west-north-west, until about the 30th degree of latitude, when their direction gradually changes towards east; and they then advance towards north-north-east or north-east until latitude 40° or 45°.

I have not thought it necessary to adduce any particular instances in support of these facts; they are well established by the observations of Redfield and Reid, and are those upon which the theory advocated by them is founded.

Now storms of high latitudes in like manner consist of two portions, but the first is from south or south-west, which depresses the barometer, and the second from north-west or north-east, which restores it to its former elevation; a calm or lull frequently intervening between them. Thus the same description applies to storms of both regions if we change the directions of the wind, they being exactly opposite.

Hence then if storms arise from the descent of the upper current of the atmosphere, its direction during tropical hurricanes must be the reverse of its general course; and it will appear from a consideration of the following facts that we have a right to infer that this is the case.

Whilst storms of high latitudes are most frequent and violent in winter, those of the tropics occur only during summer; thus of nine gales whose courses Redfield has tracked out on a chart inserted in vol. xxxi. of the American Journal of Science, one occurred in June, five in August, and three in September; in conformity therefore with general opinion,

August being usually said to be the month of hurricanes. Now the sun is at this time in the northern hemisphere, and the month of August is frequently that in which the temperature of the portion of this hemisphere north of the tropics is the highest; and although the trade-wind is still maintained, the breadth of its zone is much diminished, its southern verge retiring many degrees from the equator. We are, therefore, fully justified in supposing that the air of the regions about the tropic may become for a time so much warmer than that nearer the equator, as to reverse the usual relation of the heights of the atmospheric columns, and consequently the direction of the upper current, which now flows from north to south; although the lower strata of the air are apparently unaffected by it until the descent of this current to the surface of the earth. But we find also that Redfield and Reid have fully succeeded in identifying the West India hurricanes with the typhoons of the Chinese Sea and India Ocean, which set in from north, and occur at the same time of the year, that is, when the south-west monsoon is blowing; when of course the direction of the upper current being from north-east, or the opposite of the monsoon, is actually that of the onset of the storm, thus affording almost proof of the truth of the inference.

Let us suppose then that, in accordance with the views here advanced, the upper or northerly current falls with the velocity of a hurricane between B and C, places on the same meridian; the air rushes from B to C with the force given by its momentum without being replaced, causing a rarefaction of the air at B, and consequently a diminution of its pressure; this rarefaction increases with the continuance of the storm until the force of the wind is overcome; when, after the interval of a calm caused by the balancing of opposite forces, the density of the air south of C prevails and causes it to rush back, as into a partial vacuum, to restore the former density and pressure; the barometer now rising until the cessation of the hurricane. But whilst this is going on in the space between B and C, it is evident that the air being so rarefied at B will cause a flow towards it from north; and fresh portions of the upper current will descend on the north of B. Thus whilst the south wind or last portion of the hurricane is blowing from C towards B, the north wind or first portion is blowing from A towards B, as is indicated by the arrows; and the barometer is rising between B and C and falling between A and B; and

Fig. 1.

$$\text{N. } \underline{A \longrightarrow \quad B \quad \longleftarrow C} \text{ S.}$$

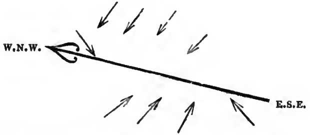
in this state of continual change the storm progresses, or rather recedes towards north. But air flowing with such rapidity from slower to quicker moving circles of latitude, and its velocity being gained in the upper regions of the atmosphere where it is little exposed to friction, will arrive with a rate of motion from west to east in the direction of that of the earth's revolution, much less than the surface of the earth upon which it falls; that is with an impulse from east to west, and hence the great easterly deflection of the north wind at the onset of the hurricane.

The effect of this latter force, however, on the progressive motion of the storm is different from that just considered, because in this case there is a constantly sustained impulse from east to west, causing it to advance in that direction, although the resistance of the air on the west of the storm and the rarefaction produced on the east of it occasion the rate of its progress to be much less than the velocity of the wind at the eastern portion of the hurricane, due to this impulse.

Thus then the progressive motion being compounded of two motions, the one towards north and the other towards west,—the latter prevailing at first, the storm advances in a direction, the resultant of these or towards west-north-west, as observed by Redfield, and which continues to be its direction until it begins to change its character on approaching the latitude of 30°.

Supposing then the course of the wind to be that above

Fig. 2.



described, its direction at the various parts of the hurricane will be somewhat as represented in fig. 2, though the easterly will be the predominating deflection of the northern current; the resistance of the air on the west may cause the wind, after some rarefaction has been produced, to become in some degree westerly on that side; an effect which will ensue in a yet greater degree on the return of the air in the second portion of the storm or southern current; to which the friction of the earth in its first progress towards south, together with the re-

sistance given to its advance towards west, will probably have been sufficient to give a western impulse on its return to portions of the earth's surface having a less velocity of revolution. On the eastern side however the air will flow towards it from the eastward of the storm, whilst a portion of the air in the centre will be calm.

If now we conceive such a mass in motion towards west-northwest as shown by the arrows, at, as is most probable, a very irregular rate on account of the occurrence of the calms, it is obvious that the veering of the wind will be that observed by Col. Reid; that at those places over which a part of the storm near the centre passes, the wind will change with the interval of a calm to south-west or south-east, whilst at those on the eastern side it will veer by east, and on the western side by west, the barometer beginning to rise as the wind becomes southerly. The barometer however has been frequently observed by W. C. Redfield to rise before the change of the wind, and so decidedly as to make him suppose the axis of the whirlwind to be in an oblique position, the higher part being in advance of the lower, which is retarded by the friction of the earth. This is obviously effected by the check given to the current to the south of the locality at which the rise takes place.

But these characters of the storm can only continue so long as the course of the upper current remains the same, which we cannot suppose it to do much beyond the tropics, and accordingly some degrees beyond that latitude both the progressive motion and the direction of the wind begin to change.

Now from the data of the two storms before alluded to, which are given only after the gales had passed the tropic, and consequently differ very materially from those given by Col. Reid of hurricanes within the tropics, we find the phænomena to be generally as follows: the progressive motion is towards north-north-east, the onset of the wind on the eastern side is most frequently from south-east, but sometimes this direction alternates with north-east, and in both cases the wind changes to north-west. On the western side the storm usually begins from north-east or north-west, but ends, as in the previous instances, from north-west.

Thus then the direction of the upper current having changed, flowing in its usual course from south to north, the storm no longer maintains the uniformity of its character, but commences at the places upon which it arrives during its progress, from three of the eight principal points of the compass, south-east, north-east, and north-west, although it always terminates with the wind from north-west; thus showing, that although the

flow of air from north is still maintained as in the tropical portion of the track of the storm, in every rarefaction it is met by the momentum of the upper current, instead of as before merely by air of greater density; and the directions of the wind are the resultants of the forces of these two currents, the south being generally predominant on the eastern side, and the north on the western: but the latter, which now must ultimately prevail to restore the equilibrium of the atmosphere, is always the one which terminates the storm, though still influenced by the descending air, which makes it blow from north-west, and which is still the principal agent in carrying on the storm, giving to its progressive motion an impulse from west.

The change in the direction of the upper current is indeed conspicuously marked in that of the progressive motion, which, being at first towards west-north-west, gradually becomes less westerly as the impulse from east diminishes, and changes altogether from west when the direction of the upper current is completely changed; hence the approach to a parabolic form of the curve described by the path of the storm, being occasioned by the gradual decrease of the impulse from east, and increase of that from west, whilst the motion towards north remains the same.

The change in the direction of the wind both at the onset and latter period of the storm, which takes place simultaneously with the change in that of its progressive motion, appears very clearly from the "data" of the second of these storms, that of August 1830 (Col. Reid's Law of Storms, p. 18): thus "in latitude 26° 51′, longitude 79° 40′ in the Florida stream, the gale was severe on the 15th from north-north east to south-west." As also "at St. Andrew's (Georgia)," latitude 30° 55′, longitude 81° 50′, "from 8 o'clock p.m. on the 15th to 2 a.m. on the 16th, the storm was from an eastern quarter, then changed to south-west and blew till 8 a.m." Thus far then the storm preserved its original characters, progressing towards north-west, and terminating with the wind from south; but at the next station for which the data of the storm are given, both had changed; thus "off Tybee and at Sawannah (Georgia)," latitude 32°, longitude 81°, it began "on the night of the 15th, changed to *north-west* at 9 a.m. on the 16th, and blew till 12 m."

As the direction of the wind at the onset of the storms of the tropics is the opposite of that of those of high latitudes, so of course the progressive motion of the latter ought to be opposite also, or towards south-east. It is not so easily traced in storms of high latitudes, but what evidence we have goes to show that this is the case. Thus W. C. Redfield says (American

Journal of Science, vol. xxxv. p. 223) "the great storm of November 29, 1836, appeared in the north of Germany after it left the shores of England, and other British storms have also exhibited an easterly progress." J. P. Espy has also traced British storms which have moved towards south of east.

But it will also follow that those regions which are visited by storms, which have arisen within the tropics and have moved from them into high latitudes, will have storms of two characters; the one progressing, as those which are the subjects of this paper, towards north-east, and the other, those originating within themselves, which move towards south of east. Such a region is America and the portion of the Atlantic adjacent to its coast; and this seems to have given rise to one part of the discordance between the observations of Espy and those of Redfield; thus the former (page 188) in quoting Redfield's language regarding the north-east progress of the storms of the United States, says, "Perhaps they sometimes move towards east or even south-east." Now as tropical storms occur only in summer, those of winter must have their origin in extra tropical latitudes, and therefore will be those moving towards south of east; and accordingly a decided instance of this kind given in Espy's volume (page 283), is that of a storm which occurred in December, and which was traced by Professor Loomis in a direction towards south of east.

[To be continued.]

XXVIII. *On the Composition of an Acid Oxide of Iron* (*Ferric Acid*). *By* J. Denham Smith, *Esq.**

In the autumn of the year before last, whilst pursuing some investigations respecting the alleged conversion of carbon into silicon, I remarked that when the residuum of the calcination in close vessels of ferrocyanide of potassium (carburet of iron) was fused with carbonate and nitrate of potash, and the resulting compound treated with water, a solution of a deep amethystine red colour was produced; this rapidly decomposed, evolving oxygen gas, and depositing sesquioxide of iron, until the decomposition being completely effected, it became quite colourless; no manganese could be detected in the solution, nor in the deposit, although the colour of the former was precisely similar to the permanganate of potash in solution†.

Remembering that the combination of an oxide of iron with potash was already on record, but unable to recall to

* Communicated by the Chemical Society; having been read May 16, 1843.

† [See Phil. Mag., S. 3, vol. xix. p. 302.]

memory the publication in which it was noticed, I am indebted to the Editors of the Philosophical Magazine for referring me to the *Journal de Pharmacie*, tom. xxvli. p. 97, where I found a communication from M. C. le Fremy, upon "The Action of the Alkaline Peroxides on Metallic Oxides." Since the appearance of this memoir, which has been followed by researches by the same author on the combination of potash with the oxides of zinc, tin, &c., various notices have been published by MM. Trommsdorff, Wackenroder and Poggendorff, on the subject of this combination of oxygen, potassium and iron, confirming the existence of this purple-red compound, and pointing out various modes of obtaining it.

I had expected that M. Fremy would have extended his inquiries and ascertained the composition of this new oxide of iron; but as a considerable period has elapsed since his original notice of its existence, and his subsequent researches in connexion with this subject having taken other directions, other chemists also having investigated the compound, and as my own experiments were commenced before I was referred to M. Fremy's paper, I may be excused for having thus directed my attention to the composition of a substance, to the priority of the discovery of which I have no claim, although I had ascertained its existence independently of the observations of the French chemist. I have entered on this explanation, as I would in nowise wish to deprive M. Fremy of his just claim to the original notice of this compound; but as my previous observations and pursuit of the subject were entirely independent of M. Fremy's notice of its existence, I feel myself justified in communicating the results of my investigations, especially as its first discoverer appears to have abandoned the pursuit.

In the memoir in the *Journ. de Pharm.* already alluded to, various modes are pointed out for preparing this combination of potash and oxide of iron, which is obtained by igniting a mixture of sesquioxide of iron, potash and nitre, or peroxide of potassium and sesquioxide of iron, or by calcining at a full red heat potash and sesquioxide of iron; by these means a brown substance was procured which afforded a deep violet-red coloured solution, very soluble in water, but which solution is easily decomposed; concentrated solutions of the alkalies precipitate it of a brown colour, which precipitate redissolves on the addition of water; it decomposes rapidly by an elevation of temperature, and instantaneously by the contact of organic substances. The same coloured solution may be formed by passing a current of chlorine through a concentrated solution of potash, holding precipitated sesquioxide of iron in

suspension. The German chemists prefer deflagrating dry nitrate of potash with half its weight of iron-filings at a temperature approaching to visible redness, and also direct the employment of chlorine gas, potash and precipitated oxide of iron, as mentioned by M. Fremy, to procure it. M. Poggendorff found that when a current from Grove's battery is passed through a solution of one part of hydrate of potash in four of water, using *cast iron* for the positive pole plunged in the potash, and wrought iron or any other convenient metal as the negative pole, ferrate of potash was formed in the solution, which immediately becomes dark red and opake. Wrought iron and steel do not produce this compound, but evolve oxygen gas; the red solution soon decomposes either with or without the continuance of the passage of the electrical current, oxygen gas being liberated and sesquioxide of iron precipitated.

To prepare this compound I have found the subjoined process attended with very uniform success:—Wash the ferri sesquioxydum of the shops with boiling water until free from sulphate of soda, dry and ignite at a low red heat; this furnishes a very pure oxide of iron and in a state of minute division; one part of this is to be intimately mixed with four of dried nitre, reduced to fine powder; place this mixture in a crucible of about twice the capacity of the bulk of the mixture, lute a well-fitting cover on, making a few small holes in the lute to allow the escape of gas, and ignite at a *full red* heat for about an hour, if six or eight ounces are made: the time of ignition depends much on the quantity prepared, and the temperature should never be raised above a full red. When well prepared it presents the appearance of a dark reddish-brown porous mass, rapidly deliquescing on exposure to the air, so that I have found it advantageous to powder it whilst still warm, when it may be preserved for use in a well-stoppered bottle, apparently for any length of time.

To examine the solution of this substance it is most convenient to employ ice-cold water, as much heat is evolved when it is thrown into water, and the decomposition of the solution is augmented or retarded by the elevation or depression of temperature. On the addition of water it evolves much oxygen gas with effervescence, probably owing to the decomposition of peroxide of potassium; if the water employed be ice-cold and the vessel containing it plunged in ice, the decomposition of the solution will be retarded. By allowing the mixture to subside for a few minutes, a solution is obtained almost free from oxide of iron in suspension, of so deep an

amethystine red colour as to be apparently opake except at the edges, but by dilution its colour is readily perceived; it gradually decomposes, evolving oxygen gas and depositing sesquioxide of iron; heat facilitates this decomposition; at 212° F. it is completely effected, and the solution remains colourless. It is decomposed, evolving oxygen, by sulphuric and nitric acids, chlorine is liberated by the addition of hydrochloric acid; by oxalic acid, carbonic acid gas mixed with oxygen is given off. It affords no precipitate with the salts of lime, magnesia, or strontian, but on the addition of a barytic salt a voluminous crimson-red precipitate falls, which may be washed, collected and dried at 212° without changing colour. With the metallic salts, the bases of which are capable of combining with more than one equivalent of oxygen, as nickel, manganese, &c., it produces the superoxide of these metals, but with the salts of zinc and metals combining with but one equivalent of oxygen, it precipitates their oxides and evolves oxygen gas.

When the deep pink-coloured solution is prepared by passing a current of chlorine gas through concentrated solution of potash holding oxide of iron in suspension, or through the deep amethystine solution already described, keeping the vessel cool during the passage of the gas, the solution obtained is of a lighter colour than the amethystine liquid and very permanent, it having been kept for months in close vessels without decomposition being completely effected, the gradual progress of which however is shown by the deposition of a light brown substance (sesquioxide of iron?). This chlorinated solution may even be evaporated and a pink salt obtained, but I have been unable to isolate by any means I have attempted the potash salt of this oxide of iron. I may add, that the characters assigned to these two solutions by the French and German chemists are generally in accordance with my own observations.

Although foiled in my endeavours to obtain the potash salt free from admixture of other salts, I was yet enabled to deduce from it the composition of this new oxide of iron, which in accordance with my suggestion, Sept. 1841, is now usually called ferric acid, and subsequently to confirm this constitution by the analysis of the barytic salt.

The mode of analysis was extremely simple, being founded on the perfect decomposition of the deep amethystine solution at 212°. A solution of the reddish-brown fused mass was prepared with ice-cold water and with the precautions before described; when the insoluble portion had subsided, which it did rapidly, and a portion of the solution had been tested by

withdrawing it by a tube and mixing with a large quantity of distilled or of lime water (which latter seems to possess the property of checking the rapidity of the decomposition of this potash salt when very dilute), to ascertain whether the subsidence of the uncombined oxide of iron was so complete that an apparently inappreciable quantity was held in suspension; a certain quantity of this clear amethystine solution was transferred to a retort of a known capacity—leaving, of course, a portion of atmospheric air in the upper part of the body and in the neck of the retort; this quantity of air was accurately determined by previous measurements and graduation of the retort; heat was then applied, and the contents of the retort raised to violent ebullition which was continued so long as any gas was evolved; these gaseous products were collected over water, and when no more gas was liberated absorption was allowed to take place by withdrawing the lamp from the retort; any air which the retort then contained was added to that already collected, and when the gas had acquired the temperature of the surrounding atmosphere, it was measured, the known bulk of atmospheric air originally contained in the neck and upper part of the retort subtracted from it, and the residue, oxygen, corrected for temperature and pressure.

The oxide of iron deposited during the boiling was dissolved in hydrochloric acid and estimated in the usual way. I subjoin the detail of an analysis as an example*. The results of several successive experiments are contained in the following table:—

Exp.	Cub. in. at 60° and 30.	Grs. of oxygen.	Grs. ox. iron.	Total wt. of oxygen.	Weight of iron.
1	12·3	4·2	16·4	9·12	11·48
2	9·88	3·37	12·6	7·15	8·82
3	13·03	4·45	16·8	9·49	11·76
4	5·21	1·78	7·2	3·94	5·04
5	14·70	5·03	22·3	11·72	15·61
6	7·58	2·59	10·6	5·77	7·42
7	33·41	11·41	42·0	24·01	29·40
	96·11	32·83	127·9	71·20	89·53

The mean of these experiments gives 22·27 parts of oxygen

* A retort filled to a certain limit with the solution left 13·75 cub. in. of atmospheric air in the body and neck of the retort, and gave 26 cub. in. of gas at a temperature of 58°, and barometer at 29·9 26 — 13·75 air = 12·25 oxygen corrected, weighs 4·2 grs. The oxide of iron freed from silica by hydrochloric acid and ammonia, gave 16·4 grs. of sesquioxide = 11·48 iron, and 4·92 oxygen + 4·2 = 9·12 oxygen combined with 11·48 iron.

by weights to 28 parts or one equivalent of iron, which is equal to a deficiency of 1·74 part in three equivalents, or 24 parts of oxygen. This is, I submit, an approximation sufficiently near to three equivalents of oxygen to one of iron, to justify me in adopting the formula FeO^3, as representing the constitution of this acid oxide of iron; especially when we consider that the circumstances under which the experiments were made would indicate the probability of some free sesquioxide of iron being suspended in the solution, as, from the unstable nature of the compound, a short period of time only could be allowed for the deposition of any insoluble matter, and the impossibility of subjecting it to filtration, owing to its decomposition by the contact of organic substances. This excess of oxide of iron varies in the different experiments quoted, so that whilst Nos. 4, 5, and 6 afford 21 to 22 parts oxygen to an equivalent of iron, 1, 2, 3, and 7 all exceed 22 of the gas to an equivalent of the metal, the last, No. 7, especially; and this experiment being the one upon which I am inclined to place the greatest reliance from the concentrated and clear state of the solution, gives nearly 23 parts (22·85) of oxygen to 28 iron. This cause, together with a certain amount of decomposition during the transference from the vessel in which the solution was made to the retort, affords an explanation of the deficiency in the quantity of oxygen as indicated in every experiment, in comparison with that which I theoretically assign to this compound.

In no one case was the quantity of oxygen actually obtained so small as to indicate a compound of 20 parts of oxygen to 28 of iron ($2\ Fe + 5O$ or $F^2 + O^2$), and I do not consider the formula intermediate with this last and the constitution I have assigned to the oxide, is a very probable one to exist, being $3\ F + 8\ O$; moreover, in four out of seven experiments more oxygen was obtained than such a compound could evolve.

The conclusion at which I arrived, that this ferric acid is a *teroxide of iron*, is confirmed by the subsequent examination of the insoluble crimson compound it forms with barytes. This salt is readily formed by adding an excess of a dilute solution of a barytic salt to the clear amethystine solution, which, prepared as I have described and dissolved in close vessels, will not contain the sulphates in any marked quantity, nor any carbonic acid whatever, the precipitate being washed in vessels excluded from the atmosphere with freshly boiled distilled water, to prevent the access of carbonic acid to the alkaline solution, and drying at 212° F. It is of a dark crimson red colour, decomposable before drying by the mineral

acids, including carbonic acid, but less readily by sulphuric acid than by the others. When strongly heated it evolves water and oxygen gas. As diluted nitric acid appeared to decompose this salt without the formation and evolution of any but oxygen gas, I selected this acid as the agent of analysis.

Having procured a very thin light flask with a long narrow neck of the capacity of about four fluid ounces, it was about half filled with dilute nitric acid, which, together with the flask, weighed 1862·05 grs., to this 33·16 grs. of the red barytic salt were gradually added, a rapid evolution of oxygen ensued with every addition of the barytic ferrate; at the expiration of twenty-four hours the flask and contents were weighed = 1891·94 grs., which indicates a loss of 3·27 grs. of oxygen gas; the solution was of a light pink colour, showing that decomposition was not wholly effected; but this entirely disappeared, and the solution became colourless on the addition of two drops of weak hydrochloric acid to the warm solution, evidencing so small an amount of the undecomposed salt as not to be worth considering: this solution evaporated to dryness, redissolved, and filtered, gave 2·78 grs. of silica and sulphate of barytes. On the addition of sulphate of soda to the solution sulphate of barytes was precipitated, which, washed, dried and ignited, weighed, exclusive of ash of filter, 24·64 grs.; the oxide of iron precipitated by ammonia gave 8·88 grs., leaving 2·09 for water and loss.

A second experiment upon a portion of this salt, prepared at a subsequent period to that used in the first experiment, and adapting a tube containing chloride of calcium to the flask, gave from 47·47 grs. of the compound, 4·31 grs. of oxygen, and, exclusive of ash of filters, 0·48 grs. of silica, &c., 37·82 grs. sulphate of barytes, and 15·27 grs. of sesquioxide of iron, leaving 2·63 grs. for water and loss.

A third experiment gave 3·06 grs. oxygen, ·69 grs. of silica, &c., 27·163 grs. sulphate of barytes, and 10·86 of oxide of iron from 34·47 grs. of the red barytic salt.

To obtain the water, I merely heated the ferrate gradually and gently on a sand heat till it assumed a greenish colour, rejecting those experiments which were partially converted into a light drab colour, which is an evidence of loss of oxygen. 64·48 grs. of ferrate lost 4·93 grs., and 44·41 lost 2·91 grs.; by ignition this green residue was converted into a drab-coloured powder, apparently with the loss of half an equivalent of oxygen, which residue, treated with dilute nitric acid, evolved oxygen gas. Now, estimating the loss sustained in these experiments as water and taking the mean, ferrate of barytes when pure will contain 7·2 per cent. of water, and

the three analyses of this barytic salt should respectively indicate, if the loss sustained in these be only water, 2·18 grs. 3·38 grs., and 2·44 grs., a result, as will be seen by reference, so near the loss sustained in analysis, that the difference may fairly be reckoned as merely an error of experiment. The results obtained from the three foregoing analyses, reckoning the loss as water and deducting the silica and sulphate of barytes as impurities, will give respectively 30·38 grs., 46·99 grs., and 33·78 grs., as the quantities of pure ferrate of barytes operated upon, yielding—

	I.	II.	III.
Barytes	16·14	24·78	17·75
Sesquioxide iron .	8·88	15·27	10·77
Oxygen . . .	3·27	4·31	3·06
Water and loss .	2·09	2·63	2·20
	30·38	46·99	33·78

The mean of which will give 19·56 of barytes, 11·64 of sesquioxide of iron, 3·35 of oxygen, and 2·31 of water contained in 37·06 of ferrate barytes.

The 11·64 parts of sesquioxide of iron are composed of 8·15 of iron and 3·49 of oxygen, which uniting with the 3·55 of oxygen gas (within ·06 of a grain in 37·06 grs. of the quantity theoretically required), will yield 15·19 of teroxide of iron, or ferric acid, combined with 19·56 parts of barytes, indicating the formula BaO FO_3 HO as the composition of this salt.

	Exp.	Theory.
1 equiv. barytes . .	19·56	20·60
1 equiv. ferric acid .	15·19	14·06
1 equiv. water . .	2·31	2·4
	37·06	37·06

I therefore consider the acid oxide of iron present in the deep amethystine solution of the potash salt, and in combination with barytes in the crimson-red barytic salt, to be a teroxide of iron, containing twice the quantity of oxygen existing in the sesquioxide. Whether the pink salt obtained by the agency of chlorine is another and distinct combination of iron with oxygen, forming a still more highly oxygenated compound than ferric acid, I am at present unable to state, but I am inclined to this opinion; it is certain, however, that another acid oxide of iron exists, probably of a lower degree of oxidation than the ferric acid. The combination of this acid with potash or soda is of a beautiful emerald-green colour, precisely similar to the green manganate of potash. The conditions requisite to procure this compound I am at present unable to point out, but I have generally succeeded when I have

employed one-half the quantity of nitrate of potash, or what I am inclined to think superior, nitrate of soda, employed to prepare the amethystine salt, and exposed the mixture to a somewhat higher temperature. I have never obtained it unmixed with the purple salt, but owing to its being a much more stable compound than that is, it remains for a considerable length of time undecomposed at ordinary and even elevated temperatures, if excluded from the air. This furnishes an easy method to prove the green salt free from the amethystine salt. Its properties appear analogous to those of the amethystine salts, except with respect to its permanency; acids liberate oxygen, changing the solution to pink, which gradually disappears; chlorine rapidly converts it into the pink salt. It is capable of filtration without being entirely decomposed, but long contact with organic matters appears to resolve it into oxygen and sesquioxide of iron. I regret that at present I have not been able to obtain this green acid of iron in such a state of combination that I can accurately determine its position, but I hope during the next session of the Chemical Society to lay before its members a more detailed investigation of the pink chlorinated compound, and also of the green oxide, and to ascertain their composition and properties.

Romford.

XXIX. *Intelligence and Miscellaneous Articles.*

EXPERIMENTS AND OBSERVATIONS ON MOSER'S DISCOVERY. BY MESSRS. PRATER AND HUNT.

In the Athenæum, No. 812 (1843, p. 485), appears a paper on this subject by Mr. H. Prater, in which he proposes "to demonstrate, that the radiation discovered by Moser is not invisible light, as he supposes, nor heat, as has since been supposed." In this paper he relates experiments with regard to the nature of the substances that produce spectra, to the effect of dissimilar metals, the effect of unequal heat on the plates and coins employed, and the effect of heat generally; also as regards the distance from the plate at which images may be taken, as regards impressions on glass, as to polished surfaces not appearing capable of receiving the impressions; as regards comparative polish in metals, to solve the question which metal receives fastest, copper or silver?; as regards the effect of interposed substances, on the influence of mass, and as to the question, does the thinness of the plate exert an influence?

"Every substance I have tried," says Mr. Prater, "has produced its spectrum when left on a polished copper plate;—coins, whether of gold, silver, or copper, platinum, nickel, brass, pieces of glass, wafers (red, blue and white), peppermint or rose drops, whalebone, talc, gum, a horse-hair ring, lava from Vesuvius, Indian-rubber (but slight), and sealing wax." His experiments give him reason to con-

clude also, that on the whole, "it seems right to admit that the effect is greater when dissimilar metals are used;" but he has not been able to satisfy himself of the truth of the statement, that when the copper coin is heated, and the plate of copper kept very cool, the effect is increased. His experiments on the effect of heat generally show, it is stated,—"1st, that heat much increases the rapidity of the radiation, *even when the object is not in direct contact*; and 2nd, that it takes place much more energetically from gold and silver than from copper (a copper plate being used). They also show that a permanent * spectrum is to be considered only as a higher degree of that produced or rendered apparent by breathing.

"Heat does not seem to increase the effect of *metal* coins on glass. Neither did *long contact*." The polished surfaces not appearing capable of receiving the impressions are "talc [talc-mica], and, among the metals tried, steel to a certain extent, platinum and gold;" and experiments are described which, in the author's words, "seem almost sufficient to establish the important general principle, viz. *that the less metals are oxidable by exposure to the air, the less is their susceptibility to receive spectra.*"

Mr. Prater draws the following conclusions from his experiments as regards comparative polish in metals. "All these experiments show that the dissimilarity of metals is not of such importance as has been conceived: they show the difference wanted to produce the effect is a difference in brightness or oxidation, *i. e.* as far as a *permanent* and good impression, *showing the lettering, &c.*, is concerned; for I find when left on the plate half an hour or so, tarnished or polished metals give equally good spectra. But in this case the spectrum is only made apparent by breathing, and of course shows nothing of the lettering, &c. However, even in this case, the spectrum of the tarnished sovereign disappeared less soon by breathing on it than did that of the polished one; so in reality the spectrum of the former may be said to have been the most perfect.

"The same remark applies to a glass plate.

"*As regards the effect of interposed substances.* As every substance tried left a spectrum, I did not much expect that the influence would permeate any lamina, even of the thinnest description. Accordingly when a sovereign or shilling was left twenty-four or forty-eight hours on a piece of stiff, though very thin, paper, it gave no spectrum, but the mark of the paper was alone visible. The experiment was repeated, half the coin resting on the copper plate and half on the paper: and although it remained a fortnight in this position, the half only *in contact* with the plate was visible by breathing on the paper, leaving *its own* spectral image just as if no coin had rested on it at all.

"The same experiment was repeated with the thinnest possible layers of talc, gum, cork and whalebone, glass, plane and con-

* "By a *permanent* spectrum," Mr. Prater observes, "is always meant, in this essay, a spectrum that remains when the substances or coins are removed—not a spectrum which cannot be rubbed off by *gentle* friction, for all the above *permanent* spectra are yet soon effaced by friction."

cave*, with the same result. Each substance left its spectrum, the part where the coin rested on such layer not being at all distinguishable. The spectral image of the square piece of talc was perfect to the minutest outline, and left its straight mark under the sixpence equally well as at other points. These experiments render it clear that the effect is not due to latent light, for otherwise how could it happen that a coin does not leave a spectral image when left on *transparent* substances, glass or talc, *even a fortnight?* They also show it does not depend on heat (at least alone), for a heat of 160° soon passed through thin glass and talc, and I found it impossible to keep my finger on glass or talc so placed. Yet we have seen above that even gold left two hours on talc so heated left no spectrum, permanent or temporary. So great is the effect of interposed substances, that even a *slight tarnish* on the metal exerts a very obvious effect†. One shilling was left twenty-four hours on a polished part of the plate, and another on a part of the same slightly tarnished (but yet sufficiently bright to see oneself perfectly). A very slight image only was left in the last case, that entirely disappeared when breathed on twice, while that on the polished part of the plate remained after being breathed on twelve or fourteen times.

"A sovereign left twenty-four hours or above, tarnished, gave scarcely a perceptible spectrum, and a sixpence none at all. On such a surface a sovereign was left on two different occasions, under a penny, for three hours at a heat of 160°, and barely left a spectrum of its *outer* margin; while on a well-polished surface, at same heat, the outline of the impression also would have been left as a permanent spectrum in an hour or two."

Mr. Hunt considers that mass exercises an influence and increases the effect, but Mr. Prater could not detect this in his own experiments.

"*Does the thinness of the plate exert an influence?* A farthing (in two experiments) pressed by twelve or fourteen pounds weight on a polished piece of platinum foil, in thirty hours leaves no spectrum at all; neither did it on a fourpenny piece, or a sovereign, or half-sovereign, when kept three or four hours at 160° under the same weight. I found a spectrum could be made on nearly equally thin zinc plates (zinc foil), by leaving a sixpence on it an hour or two. Zinc not being elastic, allows the pressure to be equal. The particular chemical nature of platinum has however much to do with this effect, for I found that when a fourpenny piece or another small brass metal object was left on a highly polished lamina of steel—heated to 160° or not—a spectrum was scarcely made. That elasticity and consequent *imperfect contact* is not the sole cause of the incapacity of thin laminæ of platinum and steel, for receiving spectral images, was to me

* "With the glass the experiment was only continued forty-eight hours; with the paper, talc and cork, a fortnight, silver coin being used; with the whalebone and gum, ten days, gold coin being used."

† "One spectrum, however, may be made on another; thus after the talc had remained eight hours on heated copper-plate and left a permanent spectrum, a sovereign put on this an hour left a permanent spectrum."

rendered *probable* by observing that coins placed on a thick copper plate seldom were in *perfectly* close contact, yet gave good spectra. In order to come to a more definite conclusion on this point, I got a lamina of bright copper, even thinner, and as elastic as the platinum lamina above-mentioned. Gold or silver coins left twenty-four hours on this, gave a spectrum scarcely visible, but on leaving a half-sovereign for two or three hours on it exposed to heat of 160°, as above, and pressed down by exactly the same weight, the half-sovereign left a *permanent* spectrum very well marked indeed.

"The result of this experiment obviously shows, that, although thinness and elasticity may have some little effect, the principal cause for the formation of the spectrum is the peculiar *chemical nature* of the metal, and *that a spectrum cannot be produced on a non-oxidable metal such as platinum.* Bright silver and copper plates are well known to *tarnish* by exposure to the atmosphere (the former perhaps rather by forming a sulphuret than an oxide), but no matter how. I have also found that spectra could be formed on tin and zinc plates, both of which of course are oxidable. So on copper coated with mercury, the mercury in such case no doubt readily tarnished (see section 7, Polished surfaces not receiving spectra, *antè*, p. 226). Having decided that the effect in question is due neither to light nor heat, to what cause it may be asked is it to be ascribed?

"*Conclusions.*—1st. As *brightness* of the plate is indispensable, and with brightness must exist an *increased tendency* to tarnish, or enter into chemical combination; 2ndly, as the plate must be of an oxidable metal, and judging from the experiments with silver and copper the more oxidable the better; 3rdly, as the more perfectly the coins are cleaned and dried* the less the effect; and as a dry perspiration (so to call it) must exist in a greater or less degree on all coins, since they pass through so many hands, and as perspiration is slightly acid; 4thly, as even with *clean* coins the effect† by actual contact must be admitted, but still is greater when there is a difference in the nature ‡ of the metal; and 5thly, as when the metals are not in contact (being removed only the one-twentieth of an inch apart), no action or spectrum is evident, if the free circulation of air, and the connexion with dust be prevented;—taking all these and minor considerations into account, we come to the conclusion that the effect in question is dependent on a *chemico-mechanical action*, or what Berzelius has called *catalytic* action. No doubt it may be urged

* "Moisture much increases the effect. Thus when one surface of the shilling was rubbed over with ink, and such surface put on the copper plate and heated to 150°, a mark much more difficult to be effaced was left than when this degree of heat was applied without moisture." [Surely ordinary chemical action was exerted in this case.—EDIT. PHIL. MAG.]

† "This is equally true, as will be remembered, with regard to glass plates."

‡ "The *general* result of all the above experiments shows this, and of course an alteration of affinity from contact is far more probable when metals are different than when the same, though if one be dirty this makes it approach the nature of a different metal."

against this view, that the action takes place when the coins and plate are both heated, and hence quite dry. But this is no solid objection, for the adage 'corpora non agunt nisi sint soluta' is not true, as hundreds of examples in chemistry show. The very fact of heat itself increasing the effect is all in favour of a chemico-mechanical view, for heat increases the tendency of copper to oxygenation, and tends also to volatilize any feeble acid matter on the coins. But again, if it be said the spectrum rubs off, even *when permanent and clearly defined* (as we have shown), *and leaves a polished surface under* it,—this we admit; but still this surface has suffered an *almost imperceptible degree* of oxygenation; for so slowly does this effect take place, that it is only visible when much advanced, as will be evident to any person who watches the gradual tarnishing of copper plates. Moser's discovery shows that *very slight* chemical action is often going on, *which has been previously overlooked.*

"The chief difficulty that occurs to the above view is, that the effect takes place to a slight extent on glass: but in all my numerous experiments I have found that the effect is *much less* on glass than on well-polished copper; for in no case has a *permanent* spectrum been made on glass even by the longest contact*. It will also be remembered that I found no effect whatever produced on talc. Now the talc scratches easily, glass of course does not; but talc is probably less soluble in acids than glass; at least in my trials it did not seem at all acted on either by nitric, muriatic, or sulphuric. To be sure, you *perceive* no effect of these on glass, but it does not seem impossible but that some *very* slight effect takes place, and that the alkali of the glass is *very* feebly acted on, as glass is a *compound* body. *Contact*, at all events, may be presumed to have an influence on the affinities of one of its elements, whether there be even the *slightest* degree of decomposition or not. Now this influence is the catalytic influence; for it has been shown above, that without actual contact, *and when all dust is kept off*, neither silver nor copper, even at the one-twentieth of an inch from the glass plate, produces any effect, though kept there ninety-six hours. In consequence of this slight alteration in affinity, the parts of glass which have been in contact some time with coins or other substances, condense the breath differently from those parts which have not: hence the spectrum.

"The effect of glass, *supposing it not susceptible of a gradual change by the action of air similar to oxidation*, is rather in favour of the spectrum depending on a mechanical than a chemical action. I have in consequence ascribed the effect to a mechanico-chemical action, or a *catalytic* action, meaning thereby an action so slightly chemical as, in the present state of the science to be scarcely appreciable†. The

* "A permanent spectrum has been proved (see experiments) to be but a higher degree of an evanescent one."

† "In coming to this conclusion I have not forgotten another difficulty viz. why a well-*polished* and boiled copper produces a spectrum on copper plate. The effect, even when continued an hour or two at a heat of 160° is *very* slight, and I found it to disappear entirely by mere breathing on the plate. *Contact* then of the same metal *slightly* modifies chemical properties; such on the present view is the inference to be drawn from this fact."

attraction of glass and oxidable metallic plates for dust, &c. is very great, and is perhaps dependent on the same causes as their attraction for oxygen. Whether or not, I feel pretty well convinced, after a laborious investigation of the discovery in question, that it is not of that wonderful character that Moser and others have supposed; nor calculated to alter our ideas of vision or of the nature of light. On the contrary, I think with Fizeau (a short notice only of whose memoir I have seen), that no effect *of any consequence* is produced *where organic matters are carefully removed by boiling water and polishing*; for such is perhaps the philosopher's opinion just named, and in as far as our opinions agree he has the priority. Begun hy a purely catalytic action, it is only continued and developed in any *marvellous* degree when those circumstances are present that permit it to assume a more strictly chemical character."

In the Athenæum, No. 815 (1843, p. 557), our correspondent, Mr. Hunt, has replied to Mr. Prater's observations:—" I am not," he says, "satisfied that Mr. Prater has succeeded in *proving that the effect is due neither to light nor heat*, and I must most decidedly object to his conclusion, that the effect is due 'to a mechanico-chemical action, or a catalytic action; meaning thereby an action so slightly chemical as, in the present state of the science to be scarcely appreciable.' I have shown, in a paper published in the Philosophical Magazine for April [preceding volume, p. 270], that *chemical decomposition* could be brought about, in some metallic salts, by the juxtaposition of metallic plates; that the iodides of copper and of gold were reduced to the metallic state by being allowed to remain for a few weeks under a copper plate with a well-amalgamated surface, the salts and the plate being nearly a quarter of an inch apart. I have since that time succeeded in decomposing many other salts in a similar manner. These facts may appear to confirm Mr. Prater's idea. We must of course consider the decomposition as a chemical phænomenon: but this change is effected by the influence of an agent which bears a strong analogy to *light*, but which is separated from it by many broad distinctions. I am by no means wedded to the opinion that *heat* is the active principle. That the phænomena described by Moser and myself are accelerated by the application of heat, all my experiments render certain, and this is indeed admitted by Mr. Prater. This gentleman has erred, as it appears to me, in exposing his plates so long to the influence of a high temperature, during which both the coins and the plates were heated in an equal degree. I have shown, in the paper above alluded to, that all that is necessary to prepare a metal plate for the reception of vapours on defined spaces, is to *disturb the equilibrium of the caloric latent in the plate*. Another point, of great importance in these investigations, is entirely overlooked by Mr. Prater. In my paper on Thermography, I have stated *that the vapours of mercury and iodine attack the plate differently*. I find that many impressions which we cannot render visible by breathing on the plates, or by exposing them to the vapour of water, are developed with beautiful distinctness by the vapour of mercury: others again, which remain invisible under the influences

of the vapours of water or of mercury, are readily evoked by the vapours of iodine, bromine, or by sulphuretted hydrogen gas. It appears to me that some very curious relations are yet to be traced out between the vapours of bodies and solids on which they may be condensed."

In No. 817 of the Athenæum (1843, p. 598), Mr. Prater replies to Mr. Hunt in the following terms:—

"*Moser's Discovery.*—It is easier to demolish old theories than establish new ones; and though I must continue to think the experiments detailed in my essay on Moser's discovery *prove* the effect is neither due to heat nor light, I did not presume to say they proved the correctness of the chemico-mechanical theory proposed by myself. That theory seems to me the most satisfactory one hitherto advanced; but perhaps future experiments may require it to be modified. At present however I have met with no experiments whose results are incompatible with such theory. I have lately looked over the papers of Prof. Draper and Mr. Hunt in the *Philosophical Magazine*, published during the last three or four months; but as in none of these experiments interposed substances or screens were used, nor the necessary precaution of *boiling* as well as polishing used, I cannot see that either Fizeau's or my own experiments [are] proved, in any way, inconclusive by the results obtained by the gentlemen just mentioned. Mr. Hunt did a great service to the subject by his discovery of the power of heat to increase the effect; but as he has not shown he can decompose the iodides of copper or gold by mercurial plates placed nearly a quarter of an inch above them, *when a screen is interposed*, the probability is that the iodides merely volatilized (as Mr. Faraday some time ago proved, even mercury itself as well as many other substances, to do) at the common temperatures of the air. The effect then in question seems to be a mere chemical action produced by direct *contact*. If future experiments prove it can be done without such contact, it will then be time to think about some new mysterious agency. But even admitting Prof. Draper's very valuable experiments to countenance such an agency (at present doubtful, as Mr. Hunt himself has well attempted to show), I do not see we are at all nearer proof that such agency is concerned in Moser's images; FOR THESE CANNOT BE TAKEN AT ANY DISTANCE FROM THE PLATE WHEN POLISHING, BOILING, OR SCREENS are used; a fact that was not known when Prof. Moser or Mr. Hunt wrote their essays.

"I did not use mercury or iodine in my experiments, because I believe results equally satisfactory may be got without them if we lengthen the time of experiment."

On the subject of the foregoing discussion see also our preceding volume, p. 324; and *Scientific Memoirs*, part xi.

OBSERVATIONS ON M. MILLON'S MEMOIR ON NITRIC ACID. BY M. GAY-LUSSAC.

The researches of M. Millon were printed in a pamphlet of 47

pages, and an extract appears in the *Journal de Pharmacie*, third series, vol. ii. p. 179. M. Gay-Lussac states that the principal fact arising from the researches of M. Millon is the following :—

If to nitric acid which is too weak to act upon metals (copper for example), or of density 1·07, a little nitrate of potash or hyponitric acid be added, action immediately commences by the hyponitric acid which oxidizes and dissolves the copper; immediately afterwards the nitric acid seizes the oxide of copper and sets the hyponitric acid at liberty; but this attacking the copper, produces deutoxide of azote, which, with the nitric acid, reproduces a fresh quantity of hyponitric acid, &c. &c.; so that the nitric acid, which is quite inactive with regard to metallic copper, confines its action to dissolving the oxide of copper and the continual reproduction of hyponitric acid. In a word, the action commenced by the hyponitric acid continues and is propagated in the manner of a fermentation; pure nitric acid does not attack metals, and when it appears to attack them, it is to the nitrous acid which it contains that the action is due.

In order to ascertain whether circumstances were such as they are described by M. Millon, M. Gay-Lussac performed the following experiment :—He prepared at first nitric acid of density 1·07, like that employed by M. Millon; but having found that it readily attacked copper shavings at a temperature of about 57° Fahrenheit, it was gradually diluted down to 1·02, previously to which it acted upon copper, but was then inactive. Some concentrated sulphuric acid was then diluted with eight or nine times its volume of water; equal quantities of copper shavings were then put into two glass tubes of the same diameter, and to one of them was added inactive nitric acid, and to the other an equal volume of diluted sulphuric acid. The two tubes were immediately placed, touching each other, in cold water, to keep the temperature equal in them both. The two acids appeared quite inactive with regard to the copper, and to each of them was added a small and the same quantity of hyponitric acid, and the copper was immediately attacked in both tubes with remarkable activity; the two liquids appeared opake and frothy, on account of the great number of small bubbles which were disengaged. The action continued for several hours, and appeared to be always as strong with the sulphuric acid as with the hyponitric [nitric ?]. The quantities of copper dissolved were also nearly equal.

This experiment M. Gay-Lussac is of opinion is not very favourable to M. Millon's theory, and at any rate it requires explanation. He is of opinion that it merely proves that hyponitrous or nitrous acid, whichever it may be called, is less stable than nitric acid; that even when much diluted, it oxidizes copper and many other metals, which are afterwards dissolved by acids perfectly inactive as oxidizers. Unquestionably, on account of the great instability of hyponitric acid, it will be readily conceived that when it exists in nitric acid, it will be first decomposed; but M. Millon's opinion can hardly be admitted, that nitric acid is by itself inactive, and only becomes active on account of the presence of a small quantity of nitrous acid, which

having commenced the action propagates it in the manner of a ferment. Not only is such an action of hyponitrous acid with regard to nitric acid unnecessary, but it would be contrary to the œconomy of nature, which never proceeds so indirectly. Besides, it is too evident that nitric acid totally free from hyponitric acid may commence action on metals, either when cold or at a higher temperature, and if it begins, it may *à fortiori* continue it.

In concluding, M. Gay-Lussac observes, that he is far from denying that the researches of M. Millon on the action of nitric acid on metals contain real merit; but still he is of opinion that the greater number of the singular facts which he has observed may be readily explained without including anything anomalous.—*Ann. de Ch. et de Phys.*, Avril 1843.

ON THE ACTION OF CHLORIDES ON PROTOCHLORIDE OF MERCURY. BY MONS. A. LARVEQUE.

On the 18th of February, 45 grains of protochloride of mercury, 90 grains of chloride of sodium, and 1875 grains of distilled water were put together into a bottle. The mixture was frequently shaken to dissolve the common salt, and the reaction was continued until the next day: the protochloride of mercury did not appear to be acted upon in any appreciable degree, the supernatant liquid was perfectly limpid and not at all discoloured by hydrosulphuric acid, nor did it produce any change on the 21st; nor was it till the 25th, when the liquor was again tested, that it was turned brown by the hydrosulphuric acid; the whole of the liquid was then filtered, and afterwards repeatedly shaken with sulphuric æther; the æther was then separated and evaporated by a water-bath in a capsule, and the residue treated with a few drops of water and tried with iodide of potassium, protochloride of tin and hydrosulphuric acid, and these did not indicate any trace of bichloride of mercury.

Nevertheless the solution of common salt contained mercury in solution, since it was coloured by hydrosulphuric acid; this colour was evidently owing to protochloride of mercury, dissolved by the alkaline chloride, since if bichloride of mercury had been formed it would have been readily removed by the æther.

This experiment was repeated with the chlorides of barium, calcium, magnesium, potassium, &c. with perfectly similar results; the presence of bichloride of mercury could not in any case be detected by means of æther.

A mixture of 45 grains of protochloride of mercury, 150 of common salt, and 750 grains of water, was made on the 26th of March, and the next day the solution was rendered brown by hydrosulphuric acid, but no bichloride of mercury was afterwards obtained by the action of æther on the liquid.

The crystallized chlorides of barium, calcium and magnesium, gave much less decisive results, for in the greater number of cases no effect was produced by directly operating on the liquor, even when 45 graius of protochloride, 90 grains of the chloride, and 1875 grains of water were employed.

If instead of exposing these mixtures to common temperatures, they are heated to 212°, the results are totally different: mercury is separated, and bichloride of mercury is found in the liquid; this may be shown by agitation with æther, by evaporation and treating the residue with iodide of potassium, protochloride of tin or hydrosulphuric acid. When the liquors are concentrated, the presence of a persalt of mercury may be detected by iodide of potassium. With hydrochlorate of ammonia ebullition is quite unnecessary, the reaction taking place almost instantly, and with the aid of æther, the bichloride of mercury formed in the solution is readily separated from it. When the chlorides of potassium or sodium are rendered impure by the presence of the iodides of these bases, there is obtained not only bichloride of mercury but biniodide also.

From the above-stated experiments, and also from others of a similar tendency, M. Larveque draws the following conclusions:—

1st. That conformably to the observations of Hervy and Guibourt, and in opposition to those of Mialhe*, the protochloride of mercury is not converted into bichloride by the action of the alkaline chlorides, at common temperatures and in the proportions above stated.

2nd. That the protochloride of mercury is always converted into bichloride and metallic mercury, when these mixtures are heated to ebullition.

3rd. That hydrochlorate of ammonia, at common temperatures, converts a portion of protochloride of mercury into bichloride.

4th. That when bichloride of mercury is formed in these various mixtures, it is always easy to separate a great portion of it by means of æther.

5th. That the alkaline chlorides dissolve a small portion of protochloride of mercury, the presence of which is shown by hydrosulphuric acid.

6th. That it is important not to employ alkaline chlorides containing iodides, because in this case chloriodide of mercury is formed.—*Journal de Pharm. et de Chim.*, Juillet 1843.

SCIENTIFIC MEMOIRS.

Part XII. of the Scientific Memoirs, completing the Third Volume, contains the following Articles:—

Proposal of a new Nomenclature for the Science of Calorific Radiations, by M. Melloni.—Memoir on the Constitution of the Solar Spectrum, presented to the Academy of Sciences at the Meeting of the 13th of June 1842, by Edmond Becquerel. With a plate.—Considerations relative to the Chemical Action of Light, by M. Arago.—On the Action of the Molecular Forces in producing Capillary Phænomena, by Professor Mossotti.—Note on a Capillary Phænomenon observed by Dr. Young, by Professor Mossotti.—Explanation of a Method for computing the Absolute Disturbances of the Heavenly

* M. Mialhe's observations will be found in Phil. Mag. S. 3. vol. xxi. p. 320, 492; and vol. xxii. p. 75. See also the late Mr. Hennell's experiments, as noticed in Phil. Mag. S. 1. vol. lxv. p. 226. Edit.

Bodies, which move in Orbits of any Inclination and Elliptic Eccentricity whatever, by M. Hansen, Director of the Observatory at Gotha.—Results of the Magnetic Observations in Munich during the period of three years, 1840, 1841, 1842, by Dr. J. Lamont. With a plate.—Observations of the Magnetic Inclination at Göttingen, by Professor C. F. Gauss.—Sketch of the Analytical Engine invented by Charles Babbage, Esq., by L. F. Menabrea of Turin, Officer of the Military Engineers: with copious Notes by the Translator.

The rules prescribed for the publication of the Foreign Scientific Memoirs prevented the Editor from inserting in that work the following authorized statement of the facts connected with the history of Mr. Babbage's Calculating Engines. As those facts may be interesting to our readers, we take this opportunity of communicating them.

ADDITION TO THE MEMOIR OF M. MENABREA ON THE ANALYTICAL ENGINE. SCIENTIFIC MEMOIRS, VOL. III. PART XII. P. 666.

Much misapprehension having arisen as to the circumstances attending the invention and construction of Mr. Babbage's Calculating Engines, it is necessary to state *from authority* the facts relating to them.

In 1823, Mr. Babbage, who had previously invented an Engine for calculating and printing tables by means of *differences*, undertook, at the desire of the Government, to superintend the construction of such an Engine. He bestowed his whole time upon the subject for many years, refusing for that purpose other avocations which would have been attended with considerable pecuniary advantage. During this period about £17,000 had been expended by the Government in the construction of the Difference Engine. A considerable part of this sum had from time to time been advanced by Mr. Babbage for the payment of the workmen, and was of course repaid; but it was never contemplated by either party that any portion of this sum should be appropriated to Mr. Babbage himself, and in truth not one single shilling of the money was in any shape whatever received by Mr. Babbage for his invention, his time, or his services, a fact which Sir Robert Peel admitted in the House of Commons in March 1843.

Early in 1833 the construction of this Engine was suspended on account of some dissatisfaction with the workmen, which it is now unnecessary to detail. It was expected that the interruption, which arose from circumstances over which Mr. Babbage had no control, would be only temporary. About twelve months after the progress of the Difference Engine had been thus suspended, Mr. Babbage discovered a principle of an entirely new order, the power of which over the most complicated arithmetical operations seemed nearly unbounded. The inven-

tion of simpler mechanical means for executing the elementary operations of that Engine, now acquired far greater importance than it had hitherto possessed.

In the Engine for calculating by differences, such simplifications affected only about a hundred and twenty similar parts, while in the new, or Analytical Engine, they might affect several thousand. The Difference Engine might be constructed with more or less advantage, by employing various mechanical modes for the operation of addition. The Analytical Engine could not exist without inventing for it a method of mechanical addition possessed of the utmost simplicity. In fact it was not until upwards of twenty different modes for performing the operation of addition had been designed and drawn, that the necessary degree of simplicity required for the Analytical Engine was ultimately attained.

These new views acquired great additional importance from their bearings upon the Difference Engine already partly executed for the Government; for if such simplifications should be discovered, it might happen that the Analytical Engine would execute with greater rapidity the calculations for which the Difference Engine was intended; or that the Difference Engine would itself be superseded by a far simpler mode of construction.

Though these views might, perhaps, at that period, have appeared visionary, they have subsequently been completely realized.

To have allowed the construction of the Difference Engine to be resumed while these new views were withheld from the Government, would have been improper; yet the state of uncertainty in which those views were then necessarily involved, rendered any written communication respecting their probable bearing on that engine a matter of very great difficulty. It therefore appeared to Mr. Babbage that the most straightforward course was to ask for an interview with the head of the Government, and to communicate to him the exact state of the case. Various circumstances occurred to delay, and ultimately to prevent that interview.

From the year 1833 to the close of 1842, Mr. Babbage repeatedly applied to the Government for its decision upon the subject. These applications were unavailing. Years of delay and anxiety followed each other, impairing those energies which were now directed to the invention of the Analytical Engine. This state of uncertainty had many injurious effects. It prevented Mr. Babbage from entering into any engagement with other Governments respecting the Analytical Engine, by which he might have been enabled to employ a greater number of assistants, and thus to have applied his faculties only to the highest departments of the subject, instead of exhausting them on inferior objects, that might have been executed with less fatigue by other

heads. It also became necessary, from motives of prudence, that the heavy expense incurred for this purpose should be spread over a period of many years. This consideration naturally caused a new source of anxiety and risk, arising from the uncertain tenure of human life and of human faculties,—a reflection ever present to distract and torment the mind, and itself calculated to cause the fulfilment of its own forebodings.

Amidst such distractions the author of the Analytical Engine has steadily pursued his single purpose. The numberless misrepresentations of the facts connected with both Engines have not induced him to withdraw his attention from the new Invention; and the circumstance of his not having printed a description of either Engine has arisen entirely from his determination never to employ his mind upon the *description* of those Machines so long as a single difficulty remained which might *limit* the *power* of the Analytical Engine. The drawings, however, and the notations have been freely shown; and the great principles on which the Analytical Engine is founded have been explained and discussed with some of the first philosophers of the present day. Copies of the engravings were sent to the libraries of several public institutions, and the effect of the publicity thus given to the subject is fully proved by its having enabled a distinguished Italian Geometer to draw up from these sources an excellent account of that Engine*.

Throughout the whole of these labours connected with the Analytical Engine, neither the Science, nor the Institutions, nor the Government of his Country have ever afforded him the slightest encouragement. When the Invention was noticed in the House of Commons, one single voice† alone was raised in its favour.

During nearly the whole of a period of upwards of twenty years, Mr. Babbage had maintained, in his own house, and at his own expense, an establishment for aiding him in carrying out his views, and in making experiments, which most materially assisted in improving the Difference Engine. When that work was suspended he still continued his own inquiries, and having discovered principles of far wider extent, he ultimately embodied them in the Analytical Engine.

The establishment necessary in the former part of this period for the actual construction of the Difference Engine, and of the extensive drawings which it demanded, as well as for the formation of those tools which were contrived to overcome the novel

[* Of M. Menabrea's treatise, which appeared in the Bibliothèque Universelle de Génève for October last, a translation is given in the 12th Part of the Scientific Memoirs just published, with copious and valuable explanatory Notes by the Translator.—Ed.]

† That of Mr. Hawes, Member for Lambeth.

difficulties of the case, and in the latter part of the same period by the drawings and notations of the Analytical Engine, and the experiments relating to its construction, gave occupation to a considerable number of workmen of the greatest skill. During the many years in which this work proceeded, the workmen were continually changing, who carried into the various workshops in which they were afterwards employed the practical knowledge acquired in the construction of these machines.

To render the drawings of the Difference Engine intelligible, Mr. Babbage had invented a compact and comprehensive language (the Mechanical Notation), by which every contemporaneous or successive movement of this Machine became known. Another addition to mechanical science was subsequently made in establishing principles for the *lettering* of drawings; one consequence of which is, that although many parts of a machine may be projected upon any plan, it will be easily seen, by the nature of the letter attached to each working point, to which of those parts it really belongs.

By the means of this system, combined with the Mechanical Notations, it is now possible to express the forms and actions of the most complicated machine in language which is at once condensed, precise and universal.

At length, in November 1842, Mr. Babbage received a letter from the Chancellor of the Exchequer, stating that Sir Robert Peel and himself had jointly and reluctantly come to the conclusion that it was the duty of the Government, on the ground of expense, to abandon the further construction of the Difference Engine. The same letter contained a proposal to Mr. Babbage, on the part of Government, that he should accept the whole of the drawings, together with the part of the Engine already completed, as well as the materials in a state of preparation. This proposition he declined.

The object of the ANALYTICAL ENGINE (the drawings and the experiments for which have been wholly carried on at Mr. Babbage's expense, by his own draftsmen, workmen and assistants) is to convert into numbers all the formulæ of analysis, and to work out the algebraical development of all formulæ whose laws are known.

The present state of the Analytical Engine is as follows:—

All the great principles on which the discovery rests have been explained, and drawings of mechanical structures have been made, by which each may be carried into operation.

Simpler mechanisms, as well as more extensive principles than were required for the Difference Engine, have been discovered for all the elementary portions of the Analytical Engine, and numerous drawings of these successive simplifications exist.

The mode of combining the various sections of which the Engine is formed has been examined with unceasing anxiety, for the purpose of reducing the whole combination to the greatest possible simplicity. Drawings of almost all the plans thus discussed have been made, and the latest of the drawings (bearing the number 28) shows how many have been superseded, and also, from its extreme comparative simplicity, that little further advance can be expected in that direction.

Mechanical Notations have been made both of the actions of detached parts and of the general action of the whole, which cover about four or five hundred large folio sheets of paper.

The original rough sketches are contained in about five volumes.

There are upwards of one hundred large drawings.

No part of the construction of the Analytical Engine has yet been commenced. A long series of experiments have, however, been made upon the art of shaping metals; and the tools to be employed for that purpose have been discussed, and many drawings of them prepared. The great object of these inquiries and experiments is, on the one hand, by simplifying as much as possible the construction, and on the other, by contriving new and cheaper means of execution, at length to reduce the expense within those limits which a private individual may command.

METEOROLOGICAL OBSERVATIONS FOR JULY 1843.

Chiswick.—July 1. Overcast: clear. 2. Overcast throughout. 3. Fine. 4. Uniform haze: very fine. 5. Sultry: very hot. 6, 7. Cloudy and fine. 8. Cloudy: rain: clear. 9. Foggy: very fine. 10. Cloudy and fine. 11. Thickly overcast. 12. Very fine. 13. Light haze: rain. 14. Densely overcast: very fine. 15—17. Very fine. 18. Very fine: constant heavy rain after 3 P.M. 19. Cloudless in the morning: cloudy at noon: clear. 20. Very fine: slight rain: overcast: rain at night. 21. Cloudy and fine. 22. Rain. 23. Cloudy and squally: cold rain. 24. Clear: cloudy and fine: clear. 25. Overcast and fine: clear. 26. Overcast: slight rain. 27. Showery. 28. Cloudy and fine: rain. 29. Very fine. 30. Cloudy and fine: clear. 31. Hazy: cloudy and fine: clear.—Mean temperature of the month 1°·3 below the average.

Boston.—July 1. Fine. 2. Fine: rain early A.M. 3. Windy. 4. Fine. 5. Cloudy: heat 81°·5 4 o'clock P.M. 6. Windy: rain early A.M. 7. Fine: thunder-storm 5 P.M.: rainbow 6 P.M. 8. Rain: rain early A.M.: rain P.M. 9. Fine. 10. Cloudy. 11. Cloudy: rain P.M. 12. Fine: rain P.M. 13, 14. Cloudy: rain early A.M. 15, 16. Fine. 17. Cloudy. 18. Windy: rain P.M. 19. Fine. 20. Fine: rain P.M. 21. Fine. 22. Cloudy: rain P.M. 23. Windy: rain P.M. 24, 25. Fine. 26. Cloudy. 27. Windy: rain early A.M. 28. Cloudy. 29. Cloudy: rain early A.M. 30. Cloudy: rain P.M. 31. Fine: rain, with thunder and lightning P.M.

Sandwick Manse, Orkney.—July 1. Bright: rain. 2. Drizzle. 3. Bright: cloudy. 4. Bright: shower. 5. Bright: damp. 6. Thunder: fog: cloudy. 7. Bright: cloudy. 8—10. Bright: clear. 11. Clear. 12. Cloudy: damp. 13. Damp: cloudy. 14. Cloudy. 15, 16. Shower: cloudy. 17. Bright: cloudy. 18, 19. Showery. 20. Rain: cloudy. 21. Bright: clear. 22. Bright: cloudy. 23. Cloudy. 24. Bright: clear. 25. Bright: cloudy. 26. Cloudy: showers. 27. Showers: cloudy. 28. Bright: cloudy. 29. Cloudy: rain. 30, 31. Damp: drizzle.

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary,* Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse,* DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month. 1843. July.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¼ a.m.	Barometer. Orkney, Sandwick. 8¾ p.m.	Thermometer. London: R.S. Fahr. 9 a.m.	Thermometer. London: R.S. Self-reg. Mx.	Thermometer. London: R.S. Self-reg. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumf.-shire. Max.	Thermometer. Dumf.-shire. Min.	Thermometer. Orkney, Sandwick. 9¼ a.m.	Thermometer. Orkney, Sandwick. 8¾ p.m.
1.	30·038	30·002	29·982	29·44			29·76	29·56	64·3	74·3	54·0	69	52	63·5			54½	52
2.	30·018	29·995	29·964	29·31			29·53	29·52	62·8	69·6	56·0	73	58	66			57	53½
3.	30·036	29·992	29·967	29·32			29·36	29·63	65·8	86·3	60·8	76	59	69			62	53
☽ 4.	30·040	29·975	29·836	29·42			29·80	29·87	59·7	76·0	58·8	78	53	66			57½	53
5.	29·798	29·717	29·643	29·19			29·83	29·73	77·8	81·6	60·3	88	55	65			57	52
6.	29·842	29·915	29·751	29·13			29·45	29·62	64·4	85·6	59·0	73	51	61			57	54
7.	30·014	29·946	29·913	29·32			29·84	29·90	64·4	84·6	56·8	68	53	63·5			55	53
8.	29·940	29·941	29·876	29·37			29·98	30·12	61·8	69·3	56·6	62	45	59			58	53½
9.	30·016	29·977	29·964	29·46			30·15	30·17	58·4	65·7	51·6	73	47	67			57	55
10.	30·018	29·984	29·935	29·44			30·17	30·20	64·7	74·8	55·0	69	55	60			56	54
○ 11.	30·080	30·160	30·009	29·54			30·24	30·24	58·3	70·3	56·2	62	52	57			56	52¾
12.	30·236	30·147	30·085	29·69			30·06	30·04	61·3	65·2	54·0	77	56	63			62	54½
13.	30·100	30·046	30·022	29·50			30·07	30·07	66·7	76·2	60·0	67	54	60·5			55½	52¾
14.	30·130	30·059	30·048	29·56			30·03	29·93	57·2	70·5	56·3	76	53	59			58	57
15.	30·136	30·117	30·048	29·48			29·82	29·93	66·7	83·3	57·7	77	56	67			55½	54
16.	30·168	30·234	30·191	29·61			29·95	30·04	66·4	76·4	61·2	80	58	64·5			56	53
17.	30·318	30·242	30·143	29·59			29·97	29·90	69·2	76·2	60·7	81	57	69			55½	53
18.	30·036	29·975	29·784	29·26			29·72	29·67	66·8	80·8	61·2	75	49	66·5			53	52
☾ 19.	29·822	29·753	29·709	29·20			29·70	29·64	61·7	76·0	53·6	68	44	63			52	52
20.	29·838	29·756	29·690	29·22			29·44	29·47	58·7	71·2	50·3	67	54	59			50½	50
21.	29·738	29·818	29·682	29·12			29·54	29·73	63·3	67·3	57·2	66	55	62			51	52
22.	29·862	29·808	29·613	29·25			29·67	29·51	64·0	70·6	57·6	65	52	60			56½	52
23.	29·516	29·781	29·457	28·90			29·70	29·86	56·7	67·0	55·8	64	40	59			52	50
24.	30·088	30·128	30·016	29·52			29·99	30·11	58·8	64·4	48·6	66	42	58·5			52	50
25.	30·246	30·221	30·176	29·68			30·15	30·15	57·9	65·3	50·6	72	47	62			56	53
26.	30·304	30·249	30·171	29·70			29·99	29·86	66·2	67·8	55·9	72	56	67			63	54
● 27.	30·104	30·055	29·995	29·43			29·84	29·94	60·8	74·2	59·0	74	54	65			54	54
28.	30·090	30·002	29·955	29·47			29·92	29·69	61·9	70·4	57·8	73	58	60			55	53
29.	29·806	29·759	29·634	29·13			29·43	29·41	65·2	74·3	58·2	71	53	64			59	52½
30.	29·702	29·676	29·636	29·06			29·54	29·70	62·7	73·4	56·4	71	49	58			55½	51
31.	29·872	29·929	29·792	29·37			29·77	29·74	60·6	70·6	54·0	69	47	61			52	51½
Mean.	29·908	29·978	29·892	29·37			29·819	29·836	63·1	73·5	56·3	71·61	52·06	62·7			53·83	52·79

The Meteorological Observations from Applegarth Manse, Dumfries-shire, for July, have not yet been received.

Days of Month. 1843. July.	Wind. London: R.S. 9 a.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. London: R.S. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.	Dew-point. Lond. R.S. 9 a.m.
1.	w.	e.	w.		ssw.	......	......	......		......	56
2.	s.	sw.	w.		sw.	......	......	·22		·45	59
3.	s.	sw.	w.		sw.	......	......	......		·09	62
☽ 4.	n.	s.	w.		sw.	......	......	......		......	61
5.	se.	se.	calm		ese.	......	·16	......		......	66
6.	s.	sw.	sw.		se.	·255	......	·03		·26	62
7.	sw.	sw.	calm		nw.	......	·14	......		......	57
8.	e.	se.	calm		ese.	·216	·24	·41		......	56
9.	nw.	n.	calm		se.	·383	·02	·14		......	58
10.	nnw.	nw.	n.		nw.	·058	......	......		......	57
○ 11.	nw.	n.	ne.		w.	......	......	......		......	58
12.	nw.	nw.	calm		sw.	......	......	·09		......	57
13.	s.	sw.	calm		wnw.	......	·07	·32		·04	61
14.	nnw.	ne.	calm			·297	......	·06		......	58
15.	s.	sw.	calm		w.	......	......	......		·05	61
16.	w.	w.	nw.		wsw.	......	......	......		......	62
17.	s.	w.	w.		w.	......	......	......		......	64
18.	w.	sw.	w.		w.	......	·50	......		......	63
☾ 19.	nw.	w.	calm		n.	·477	·02	·34		·23	58
20.	w.	sw.	w.		nw.	......	·01	......		·65	56
21.	w.	w.	w.		n.	......	......	·06		·02	57
22.	s.	sw.	calm		nw.	......	·08	......		......	58
23.	w.	w.	w.		nnw.	·065	·06	·18		......	58
24.	w.	w.	calm		nnw.	·211	......	·07		·05	56
25.	w.	sw.	calm		se.	......	......	......		......	53
26.	s.	sw.	calm		s.	......	·10	......		......	55
● 27.	sw.	w.	w.		w.	·007	·02	·10		·07	59
28.	w.	w.	nw.		w.	·116	·06	......		......	55
29.	s var.	sw.	w.		var.	·019	·07	·30		·09	60
30.	s.	w.	w.		n.	·152	·07	......		·86	59
31.	w.	sw.	nw.		nw.	......	·03	·08		·06	58
Mean.						Sum. 2·347	1·67	2·40		2·92	Mean. 59

THE

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OCTOBER 1843.

XXX. *On the new metals, Lanthanium and Didymium, which are associated with Cerium; and on Erbium and Terbium, new metals associated with Yttria.* By Professor C. G. Mosander*.

ALTHOUGH in consequence of the imperfect nature of the results which were obtained from my researches on cerium and lanthanium I had no intention of making any communication on the subject on the present occasion, yet after hearing the interesting statement of Professor Scheerer, it appeared to me that it might be useful to make known more generally some particulars which arose during my labours, and principally because this advantage may result, that other chemists, after becoming acquainted with what I am about to state, may possibly be spared the loss of valuable time which might otherwise have been fruitlessly expended.

When sixteen years since I made some experiments upon cerium, several circumstances occurred which led me to the supposition that oxide of cerium was accompanied by some other oxide, which, however, I did not succeed in separating, and want of materials prevented me from then prosecuting the inquiry. A few years since, having procured a quantity of cerite and cerine, I prepared from thence the double salt of sulphate of the oxide of cerium with sulphate of potash, which salt was washed with a solution of sulphate of potash, until the passing fluid gave no trace of precipitate with caustic ammonia or carbonate of soda. I believed that in this manner I could obtain a pure salt free from all foreign substances,

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The double salt was afterwards decomposed in the moist way with carbonate of soda, and with the carbonate of protoxide of cerium thus obtained, all the preparations have been made which will be now mentioned.

After a long examination of various salts of protoxide of cerium, I did not succeed in detecting a salt principally consisting of the supposed new oxide, the presence of which, however, appeared more and more probable in the course of the experiments. As it was known that cerium gives two oxides, I considered it probable that if hydrate of protoxide of cerium mixed with water was exposed to the effect of chlorine, peroxide of cerium would be formed while the more electro-positive metallic oxide would be dissolved in the fluid, and it was in this manner that I succeeded to my satisfaction. When the chlorine was introduced into the fluid, the appearance of the hydrate of protoxide of cerium began soon to change, the volume diminished, and a heavy, bright, yellow, or rather orange-yellow coloured powder fell to the bottom. If, after the chlorine no longer appears to cause any change, the fluid is filtered, a colourless solution, with the strong odour of hypochlorous acid, is obtained, from which, with hydrate of potash in excess, a precipitate is deposited, which collected on a filter, is white, or approaching violet. This precipitate begins soon, however, to grow yellow in contact with the air. If the precipitate be again mixed with water and chlorine introduced, the greater part is dissolved, while a new portion of the yellow-coloured oxide is formed, and remains undissolved. The filtered solution forms a precipitate again with caustic potash, which is treated as before with chlorine, and this is repeated five or six times, when, finally, hydrate of potash precipitates from the solution an oxide which does not become in the least yellow by exposure to the air, and which suspended in water, is completely dissolved by the introduction of chlorine without leaving a trace of undissolved yellow oxide. It was to this oxide, not capable of being more oxidized either by the air or chlorine, that I gave the name of oxide of Lanthanium, after the production of which, and a nearer acquaintance with its properties, another and simpler method was employed to obtain it. The strong basic qualities of the new oxide afforded an easy means of separating it from oxide of cerium, by treating the red-brown oxide which is obtained when the so-called nitrate of protoxide of cerium is heated with nitric acid diluted with 75 to 100 parts of water. An acid thus diluted leaves the greater part of the red-brown oxide undissolved, and from the solution thus obtained the oxide of lanthanium was derived which was employed by me in the experiments that I made in the beginning of the year 1839.

Some of the results which I obtained unfortunately became known to the public. When we find the oxide of a body hitherto unknown, nothing, generally speaking, is easier than the determination of the qualities of the body, and I therefore expected to be able to give a complete account of my experiments in a very short time, but on this point I was much deceived. That which, in the first place, gives any value to chemical investigation, is the certainty that the object investigated is pure, that is to say, free from foreign substances. I had not made much progress in the details of my inquiry, when it appeared that what I at first considered to be pure oxide of lanthanium, was, in point of fact, a mixture of the new oxide with a number of other substances, so that in the course of the experiments I succeeded in separating no less than seven different substances, one after the other. The first, to my great surprise, was lime, in no considerable quantity; and I have found that sulphate of lime and sulphate of potash forms a double salt sparingly soluble. Afterwards the following oxides were successively separated, and by the application of different means, namely, oxide of iron in large quantities, of copper, tin, nickel, cerium, and something resembling uranium, &c.; but even the oxide which remained after the separation of all these substances, left me in nearly the same position which I held at the commencement of the researches, so that, although at the end of the year 1839 I had already been fortunate enough to obtain oxide of lanthanium tolerably pure, it was not until the beginning of the following year that I was able, with any facility, to obtain a larger quantity of it; but, notwithstanding all my efforts, I have not yet succeeded in discovering any method of separating, with any degree of analytical accuracy, lanthanium from cerium, &c.

Oxide of lanthanium, as pure as I have hitherto been able to obtain it, possesses the following properties:—It is of a light salmon colour, or nearly white, but not in the least reddish or brown, and retains its appearance unchanged when heated either in open or close vessels at a red or white heat: the slight colour seems to proceed from a small remnant of some foreign substance. The oxide, although just previously ignited to a white heat, soon changes its appearance in water, becomes snow-white, more bulky, and after twenty-four hours in the ordinary temperature of the air, becomes changed to a hydrate easily suspended in water. With boiling water this change takes place very quickly, and begins immediately; the newly heated oxide as well as the hydrate immediately restores the blue colour to moist reddened litmus paper. Oxide of lanthanium is easily dissolved by acids even much diluted. Salts,

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when they are formed by the combination of the oxide of lanthanium with uncoloured acids, are absolutely colourless, as well as the most concentrated solutions of the same. Salts of lanthanium have a sweet, slightly astringent taste, and the solution of them can be completely separated from oxide of lanthanium by the addition of sulphate of potash in sufficient quantity, because the double salt formed by sulphate of oxide of lanthanium and sulphate of potash is quite insoluble in a solution saturated with sulphate of potash. The atomic weight of oxide of lanthanium, as it has hitherto appeared in most instances, has oscillated about 680, a number which, however, possesses no scientific value, when, as I have already remarked, an absolutely pure oxide has not yet been obtained.

Of the salts produced, I will only briefly describe a few of the most characteristic. Sulphate of oxide of lanthanium crystallizes in small six-sided prisms terminated by six-sided pyramids, containing three atoms of water of crystallization. This salt has the same property as sulphate of yttria, thorina, and other oxides of the same class, namely, being much less soluble in warm than in cold water. At 73°·4 Fahr. one part of anhydrous sulphate of oxide of lanthanium requires 42½ parts of water to be dissolved, but of boiling water one part of the same salt requires about 115 parts.

The crystals are very slowly dissolved, but the anhydrous salt is immediately dissolved. The anhydrous salt developes much heat when mixed with a little cold water, and the salt then forms a crystalline crust, which afterwards is very slowly dissolved. If powdered sulphate of oxide of lanthanium be thrown into water whose temperature is 35°·6 or 37°·4 Fahr., and kept stirring, and with the precaution that the liquid, which besides should be cooled from the outside, never attains a higher temperature than 55°·4 Fahr., one part of sulphate of oxide of lanthanium may be dissolved in less than six parts of water, and the solution preserved unchanged for weeks, in closed vessels, and within the stated limits of temperature; but if the liquid be gradually heated, then before the temperature has reached 86° Fahr., a number of crystalline groups composed of small needles radiating from a common centre begin to deposit, and when once this crystallization has commenced it cannot be checked, however rapidly we may cool the liquid. With regard to the number and form of the deposited groups, the originally clear liquid is changed in a few minutes to a thin pap. If during the dissolution of the salt according to the manner stated, a part of the liquid acquires a higher temperature through the heat that is developed by the union of the salt with water, the crystallization

of a part of the salt immediately begins, and after that has once begun the phænomenon continues even with so low a degree of heat as 55°·4 to 57°·2 Fahr. less, until the solution only contains $\frac{2}{87}$ths of its weight of anhydrous salt. The salt which has thus been deposited contains the same quantity of water as that which is formed during the evaporation, as well under 55°·4 Fahr. as with 212° Fahr. If sulphate of oxide of lanthanium be kept at a white heat for an hour, it loses the half of its sulphuric acid, and the basic salt which is produced is insoluble in water.

Nitrate of oxide of lanthanium is a salt easily soluble in water or alcohol, and from an evaporated solution of the consistence of thin syrup, it crystallizes in large prismatic crystals, which rapidly deliquesce in damp air. If the solution be evaporated with a heat of 86° Fahr. and above, an opake milk-white mass is obtained. If the salt be cautiously heated so that all the water is expelled, then by care with a higher degree of temperature, the anhydrous salt may be melted without decomposition, and after cooling it resembles a colourless glass, but with the least inattention respecting the temperature, a part of the nitric acid is expelled, and the melted mass is a mixture of neutral and basic salt, which stiffens to a snow-white opake mass, which a moment after solidification has the remarkable quality of falling asunder into a voluminous white powder, with such violence, accompanied by a sort of slight detonation, that parts of it are thrown about to the distance of several inches.

Oxide of lanthanium has a particular tendency to form basic salts, and only such are precipitated with caustic ammonia; let this be added in as great an excess as may be, when also it occurs that the combination with some organic acids, such as tartaric acid, is dissolved in an excess of ammonia. Several of the basic salts, for example, basic nitrate of oxide of lanthanium, and basic chloride of lanthanium, are marked by the quality that they cannot be washed upon a filter with water, which runs through of a milky colour, until no part of the precipitate remains upon the filter, and if the liquid be boiled with the precipitate which has been obtained, the whole runs immediately through the filter. If the precipitate be allowed to remain a few days wet upon the filter, it becomes changed into a neutral salt which is dissolved in the water, and carbonate of oxide of lanthanium, which remains upon the filter.

With regard to cerium, my investigations are as imperfect in their results as those upon lanthanium; I will, however, make mention of some facts which may prove interesting for the present.

The reddish-brown powder which remains after the extraction of oxide of lanthanium with dilute nitric acid, is a mixture of the oxide of cerium with oxide of lanthanium, together with all the above-named accompanying substances. I have not been able to find any good method of obtaining pure oxide of cerium; the salts of protoxide of cerium are like those of oxide of lanthanium, perfectly colourless, and with sulphate of potash the protoxide of cerium is precipitated completely from the solution. If hydrate of protoxide of cerium, precipitated by caustic potash, be collected on a filter, it immediately begins to grow yellow, and after the oxidation has proceeded as much as possible in this manner in the air, there remains after drying, opake light yellow lumps, which contain water; this being expelled by heat, leaves so-called oxide of cerium, which has not the least trace of brown, but after an hour's heating at a white heat, has a slight tinge of red. If the oxide of cerium formed in the manner stated has the slightest tinge of brown, or becomes dark after drying or heating, it proceeds from foreign substances. This yellow oxide, however, always contains protoxide of cerium, and I have not succeeded in obtaining oxide of cerium free from protoxide. The bright yellow oxide which is formed when hydrate of protoxide of cerium, either alone or mixed with hydrate of oxide of lanthanium, &c., is exposed to the action of chlorine, contains not only chlorine but even protoxide of cerium. If nitrate of protoxide of cerium be heated, a light yellow powder is obtained, from which much salt of protoxide of cerium may be extracted with nitric acid, and if this solution be again evaporated, and the dried mass heated, salt of protoxide of cerium is again obtained, and this continues even after the operation has been five times repeated. What I call oxide of cerium is, therefore, really a combination of oxide of cerium with protoxide. The ignited oxide of cerium is scarcely affected by boiling concentrated muriatic acid, still less by other weaker acids; the hydrate, on the other hand, is easily dissolved in muriatic acid, with the development of chlorine, but even after a long boiling the solution retains a yellow colour. Scarcely a trace of the hydrate of oxide of cerium is dissolved by weaker diluted acids, but it assumes a darker yellow colour, and combines with a portion of the acid employed. In the solutions of carbonated alkalies, particularly carbonate of ammonia, the hydrate of oxide of cerium is dissolved in large quantities, and the solution assumes a bright yellow colour. Peroxide of cerium in solutions which are heated to boiling, is immediately reduced by oxalic acid to protoxide of cerium, while carbonic acid is developed. By

means of warm concentrated sulphuric acid, the ignited oxide of cerium is immediately rendered soluble, in consequence of combining with the acid. Neutral sulphate of oxide of cerium is, when dry, a beautiful yellow, becomes by heating orange yellow, with a higher degree of temperature almost cinnabar red, but after cooling the bright yellow colour returns. The salt is soluble in a small quantity of water, but if the solution be heated to boiling, the greater part of the salt is deposited in the form of a tough, soft, semi-transparent, and very viscid mass. If the concentrated solution, which is red yellow, be diluted, it becomes lighter yellow, but begins immediately to grow turbid, depositing a sulphur-yellow powder, which is a basic salt requiring 2500 parts of water for its solution. With sulphate of potash, sulphate of oxide of cerium gives a beautiful yellow salt, which is altogether insoluble in a saturated solution of sulphate of potash, but the double salt cannot be dissolved in water without being decomposed and a basic salt precipitated. Notwithstanding the oxide of cerium is nearly insoluble in diluted acids, it must be remembered that intimately mixed with other easily soluble oxides, it readily passes into solution: sulphuret of cerium is of a dark brown-red colour.

The oxide of lanthanium which was first obtained by me was of a brown colour, but after having been heated to a white heat, became a dirty white; by heating in hydrogen it also lost its brown colour, although a scarcely perceptible loss of weight arose therefrom: by heating in the air, the brown colour returned.

This circumstance, together with several other phænomena which presented themselves during the examination of the properties of oxide of lanthanium, caused me to presume that the oxide of lanthanium which had been obtained was still accompanied by some unknown oxides, and it was in the beginning of 1840 that I succeeded in freeing lanthanium from that very substance which caused the brown colour. To the radical of this new oxide, I gave the name of *Didymium* (from the Greek word δίδυμος, whose plural δίδυμοι signifies twins), because it was discovered in conjunction with oxide of lanthanium. It is the oxide of didymium that gives to the salts of lanthanium and cerium the amethyst colour which is attributed to these salts; also the brown colour which the oxides of the same metals assume when heated to a red heat in contact with the air. Notwithstanding all possible care, I have not yet succeeded in obtaining the oxide in a state of purity; and I have only arrived so far as to ascertain that a constant compound with sulphuric acid can be produced by

different means, but from the quantity of water of crystallization, and other circumstances, the conclusion may be drawn that the salt is really a double salt, although I cannot at present say whether the other accompanying oxide is oxide of lanthanium, or some other. That which I now thus briefly describe as oxide of didymium is the basis in combination with sulphuric acid in that salt whose properties I will now communicate, as well as a method of obtaining it. The sulphate of oxide of didymium, prepared in different ways, is much more soluble in water than the sulphate of oxide of lanthanium. This circumstance induced me to try whether by treating the mixture of the anhydrous salts in great excess with water in small proportions, solutions could not be obtained, which, in the order they had been procured, should be richer in salts, and particularly in sulphate of oxide of didymium, while, on the contrary, what remained undissolved, should be nearly pure sulphate of oxide of lanthanium; but after having examined five successive saturated solutions, obtained from the same mixture of anhydrous salts, it was found that one part of anhydrous salt had in the first experiment been dissolved in 7·64 parts of water; in the 2nd experiment in 8·48 parts; in the 3rd experiment in 7·8 parts; in the 4th experiment in 5 parts, and in the 6th experiment in 7·44 parts of water. These remarkable proportions of salt dissolved I thus explained: during the dissimilar degrees of temperature which accidentally arise under the development of heat which takes place when, by the addition of water to the anhydrous salt, this takes up water of crystallization, salts containing unlike portions of water of crystallization, and of unlike solubility had been formed, and it was for the purpose of ascertaining the correctness of this supposition that I afterwards prepared the solution of the salts in the manner which I have already stated in describing the sulphate of oxide of lanthanium, the dissimilar solubility of which salt with different degrees of heat was in this manner discovered. If therefore the mixed salts, with a temperature which should not exceed 48°·2 Fahr., be dissolved in 6 parts of water, and the solution thus obtained afterwards heated to 104° Fahr., a quantity of light amethyst-coloured salt of lanthanium is deposited, which, by a repetition of the same treatment, after ten to fifteen operations, becomes colourless and nearly pure. The amethyst-coloured solution separated from the salt of lanthanium is evaporated to dryness, and the salt is freed from water; it is again dissolved in the before-mentioned manner, but the solution is now heated to 122° Fahr., and filtered after no more salt is deposited. The solution, now

red, is diluted with an equal weight of water, acidulated with a portion of sulphuric acid, and is evaporated in a warm place. Several kinds of crystals are now formed, many of which assume a larger size, and fall to the bottom; when only a sixth part of the liquid, which is generally yellow, remains, it is poured off, the salt crust which lies at the bottom is separated, and the collection of crystals is shaken in boiling water, which is suddenly poured off, when a number of smaller prismatic crystals accompany it. The remaining large red crystals are again dissolved in water, the solution is acidulated with sulphuric acid, evaporated in the before-named manner, and the large red crystals taken separately, when it will be found on a nearer examination that they form a mixture of two kinds: the one, which appear in the form of long, narrow rhomboidal prisms, is taken out, and the remaining large red crystals with many planes, which, according to Wallmark's measurement, appear to belong to the triklinometric system, form the salt which I call sulphate of oxide of didymium. From a solution of a salt of didymium hydrate of oxide of didymium is precipitated with hydrate of potash in excess, and collected on a filter; it has a bluish-violet colour, absorbs during washing carbonic acid from the air, and the residuum, for the most part formed of carbonate of oxide of didymium, is, after drying, light reddish violet. If this be heated to redness, the water passes off and carbonic acid is easily expelled. The oxide produced in this manner is obtained in the form of small lumps, dark brown on the surface, sometimes light brown in the fracture, of a resinous lustre, sometimes nearly black, with the lustre and appearance of dark orthite, at the same time particles are obtained of all the most dissimilar colours, so that they represent together a pattern map of all the most dissimilar kinds which are obtained of the mineral orthite, from the light red brown to the nearly black. The powder becomes light brown. If this oxide be heated to a white heat, it assumes a dirty white colour approaching gray green. Oxide of didymium is a weaker basis than oxide of lanthanium: it has no alkaline reaction, and appears not to absorb water after having been heated. It is, however, tolerably easily dissolved even in diluted acids, and the brown oxide with a development of gas. It is insoluble in carbonate of ammonia; its salts are amethyst-red, as well as the solutions of the salt, which forms no precipitate with hydrosulphuret of ammonia, unless a large quantity be added, or the liquid be heated, when the sulphuretted hydrogen is developed, and a basic salt precipitated having a slight tint of red. If the oxide be dissolved in phosphoric

salt by means of the blowpipe, the bead becomes amethyst-coloured with great tendency to violet, exactly as with a trace of titanic acid after reduction.

Oxide of didymium heated upon platina foil with carbonate of soda, melts to a gray-white mass. With regard to the salts of didymium, I shall briefly describe those which are analogous to the before-mentioned salts of lanthanium and cerium, and must at the same time mention that the basic salt of didymium which is precipitated by caustic ammonia, can be washed without passing through the filter.

The mode in which sulphate of oxide of didymium is obtained, as well as its appearance, has been already stated; this salt is readily soluble in water at the ordinary temperature of the air, although the crystals are very slowly dissolved. The anhydrous salt is at once dissolved, if before the solution it is not suffered to combine with water of crystallization. Should this occur in such a manner that the anhydrous salt is covered over (*öfver gjutes*) with a little water, the mass becomes heated, and a hard salt crust is formed, which must be reduced to powder before it can be quickly dissolved. At the ordinary temperature of the air, one part of anhydrous sulphate of oxide of didymium requires five parts of water for solution. This solution begins at 127°·4 Fahr. to deposit crystals, the number of which increases in the same degree as the temperature increases, so that the boiled solution contains only one part of anhydrous salt to 50·5 parts of water; at a low red heat an inconsiderable quantity of sulphuric acid goes off, but after an hour's exposure to a white heat, the salt loses two-thirds of its acid. With sulphate of potash, sulphate of oxide of didymium gives an amethyst-coloured double salt, which is completely insoluble in a saturated solution of sulphate of potash.

Nitrate of oxide of didymium is very soluble in water, crystallizes with difficulty; the solution evaporated to thin syrup, has a beautiful red colour, which seen in a certain direction approaches blue. If the salt be evaporated to dryness in a warm place, and heated to melting, which cannot be effected without a great portion of the nitric acid being decomposed, a red fluid is obtained, which, cooled and solidified, does not fall to powder with violence, like the corresponding salt of lanthanium, but retains its form.

I must not omit to mention on this occasion, that amongst the many other bodies which in the course of these researches I was obliged to examine, yttria also presented itself, and I have found that this earth, free from foreign substances, is perfectly colourless, and gives perfectly colourless salts: that the

amethyst colour which the salts generally present comes from didymium, I will not, however, maintain.

Addendum, July 1843.

On Yttria, Terbium and Erbium.

I published last summer a short notice of yttria, concerning which earth the following facts subsequently discovered merit attention. When I stated on the former occasion that pure yttria, as well as the salts of that base with a colourless acid, are colourless, my experiments had only gone so far as to show that all the yttria which I could procure for examination might with ease be separated into two portions, the one a stronger and colourless base, the other a weaker, which, in proportion as it was free from yttria, acquired a more intense yellow colour on being submitted to heat, and with acids gave salts of a reddish colour. I continued my examination during the following autumn and winter, and thereby was not only enabled to confirm the correctness of my former observations, but made the unexpected discovery that, as was the case with oxide of cerium, what chemists have hitherto considered as yttria, does not consist of one oxide only, but is for the most part to be regarded as a mixture of at least three, of which two appear to be new and hitherto unknown, all possessing the greater number of their chemical characters in common, for which reason chemists have so readily overlooked their real differences.

The characters which are peculiar to these oxides, and distinguish them from all others are,—1st, that although powerful salt bases, all more so than glucina, they are insoluble in water and in caustic alkalies, but on the other hand soluble, even after having been exposed to a strong heat, in a boiling solution of carbonate of soda, although after a few days the greater part separates from its solution in the form of a double salt; 2ndly, that combined with carbonic acid, they are largely soluble in a cold solution of carbonate of ammonia, and that when such solution is saturated with them, a double salt of carbonate of ammonia and the above carbonates immediately begins to separate, and that in such quantity, that after a few hours very little oxide remains in solution; which explains the observations of several chemists, that, as they express themselves, yttria sometimes dissolves freely, sometimes scarcely at all, in carbonate of ammonia: further, that the salts of these oxides have a sweet taste, and that the sulphates dissolve with more difficulty in warm than in cold water, without its following that they form double salts with

sulphate of potash, which are insoluble in a saturated solution of the latter.

If the name of yttria be reserved for the strongest of these bases, and the next in order receives the name of oxide of terbium, while the weakest be called oxide of erbium, we find the following characteristic differences distinguishing the three substances:—The nitrate of yttria is extremely deliquescent, so much so that if a small portion of a solution of that salt be left for weeks in a warm place, the salt produced will not be free from humidity. The solution of nitrate of oxide of terbium, which is of a pale reddish colour, soon evaporates, leaving a radiated crystalline mass, which does not change in air unless it be very damp. The crystals of sulphate of yttria are colourless, and remain clear and transparent for weeks in air at a temperature varying from 86° Fahr. to 158° Fahr., while a solution of sulphate of oxide of terbium yields by evaporation, at a low temperature, a salt which immediately effloresces to a white powder. Oxide of terbium, the salts of which are of a reddish colour, appears, when pure, to be devoid of colour, like yttria. Oxide of erbium differs from the two former in its property of becoming of a dark orange yellow colour when heated in contact with air, which colour it is again deprived of, with a trifling loss of weight, by heating it in hydrogen gas; and it is to the presence of oxide of erbium that yttria owes its yellow colour, when prepared as hitherto directed: and it is moreover probable, that in all those cases where a colourless yttria has been supposed to have been obtained, the presumed yttria has consisted for the most part of glucina, at least before it was known how to separate the last earth completely.

The sulphate and nitrate of the oxide of erbium are devoid of colour, although the solution of the oxide in acids is sometimes yellow, and the sulphate does not effloresce.

These and a number of other less remarkable differences between the three oxides, appear to me to place beyond a doubt that what we have hitherto obtained and described as yttria, is neither more nor less than a mixture of these three bases, at least such is the case with yttria prepared from gadolinite, cerine, cerite, and orthite, but as I have not yet had the good fortune to discover any tolerably easy or certain mode of obtaining the one or the other oxide chemically pure, I shall confine myself for the present to this short statement of facts.

I proceed to make known two easy methods by which chemists may prove the correctness of the above statements. If caustic ammonia in small quantities at a time be added to a solution of ordinary yttria in muriatic acid, and the preci-

pitate following each addition be washed and dried apart, we obtain basic salts, of which the last precipitated are colourless, and contain yttria only. Going backwards in reverse order from these last, we find the precipitates becoming nearly transparent, reddish, and containing more and more oxide of terbium, while the first precipitates contain the greatest proportion of oxide of erbium, mixed with oxide of terbium and yttria. If a solution of ordinary yttria in nitric acid be treated in the same manner, and the several precipitates be heated separately, the first precipitate will give a dark yellow oxide, the colour of each succeeding one will be paler and paler, till at last a white oxide will be obtained, consisting chiefly of yttria, with a trifling quantity of oxide of terbium. In making these experiments it is of importance that the yttria be free from iron, uranium, &c., a matter of considerable difficulty. It is therefore better to commence precipitating with a weak solution of hydrosulphuret of ammonia, and when the precipitate has no longer a shade of bluish green, then to apply the caustic ammonia as described. A better method in general is to add a portion of free acid to a solution of yttria, and then to drop in a solution of binoxalate of potash, continually stirring till the precipitate no longer redissolves. In a couple of hours a precipitate will form, which is to be separated, and the remaining solution treated as above described, and that as long as any precipitate is formed. If the remaining fluid be then neutralized with an alkali, a small quantity of nearly pure oxalate of yttria is obtained. Of the precipitates the first obtained are most crystalline, and fall quickly, the last more pulverulent, sinking slowly. The former contain most oxide of erbium, mixed with oxide of terbium and yttria; the next contain less oxide of erbium, more of terbium and yttria; while the latter contain more and more yttria, mixed with oxide of terbium. The first precipitates are always reddish, and the last colourless. If a mixture of the oxalates of these bases be treated with a very diluted acid, we obtain first a salt containing mostly yttria, then one richer in oxide of terbium, and the remainder contains principally oxide of erbium. I have even once succeeded in obtaining a double salt of sulphate of potash and sulphate of oxide of erbium (which is with difficulty dissolved in a saturated solution of sulphate of potash), by treating a somewhat concentrated solution of the nitrates of oxide of terbium and erbium with an excess of sulphate of potash.

That much time and labour have been employed in arriving even at the results which I have hitherto obtained, will be evident from the little I have been enabled to make known,

particularly when it is considered that one or two grains of yttria have often been divided into nearly a hundred precipitates, which have been individually examined; but I live in hopes that the knowledge already obtained will soon enable me to publish a more complete account of my investigations.

XXXI. *Experiments on the mutual relations of Electricity, Light, and Heat.* By Elias Wartmann, *Professor of Physics in the Academy of Lausanne.*

1. *On the relations which connect Light with Electricity, when one of the two fluids produces chemical action*.*

THE experiments of Scheele, of Ritter, of Seebeck and of Talbot on the chloride of silver, those of Wollaston on gum guaiacum, of Sir J. Herschel on the precipitation of chloride of platinum by water, and lastly, the beautiful discoveries of M. Daguerre, have placed beyond doubt the existence of a particular influence of light which has been attributed, perhaps without sufficient proofs, and by analogy, to a chemical power supposed to reside in it. On the other hand, a numerous train of electro-chemical labours from the beginning of the present century have proved, that chemical and electrical actions always accompany each other, and are, so to speak, consolidated together and mutually answerable for each other. Chemical action therefore forms a plain on which light and electricity meet. What analogies and what dissimilarities do they there present? Does one of the fluids act in the same manner in the presence or in the absence of the other? Does light become electricity, or does it disengage it when it acts chemically? Here are different questions which have hardly been touched upon. Without pretending to reply to them in a decisive manner, I have endeavoured to procure some data at least with respect to them, and such has been the aim of the following researches.

I at first sought to discover what influence electricity already produced might have on a chemical action operated simultaneously by light; for this purpose the Daguerreotype appeared to me the most convenient and at the same time the most delicate instrument. A proof was obtained after an exposure for

* [From the *Archives de l'Electricité.*] The lines which follow this title form the second part of a memoir communicated to the Society of Physics and Natural History of Geneva on the 1st of July, 1841. The title of this memoir was, "Experimental Researches on the Imponderable Fluids." The part which I now publish treats of matters in the order of the day, and which are not without some connection with the fact mentioned at the end of the preceding article. The same work had been communicated to the Society of the Natural Sciences of the Vaudois (*Verhandlungen der Schweitz. naturforsch. Gesellschaft* bei ihrer Versammlung in Zurich, 1841, p. 272).

ten minutes in the camera; it was very beautiful. Immediately afterwards, a second was made by submitting the iodized plate to the intense current of a pile of twenty elements* during the entire continuance of the action of light upon it.

After the lapse of fourteen minutes, which were judged necessary because the sky had become overcast, the Daguerreotyped drawing was found without fault, without any striation, or anything indicating that the current had acted in the direction of the line connecting the points of contact of the conductors, or in a direction parallel, oblique or perpendicular to it†.

Thus an iodized leaf of silver plate is not altered in its chemical nature by the passage of a strong voltaic current; the iodine is not volatilized, and the iodide remains quite as sensible to the action of light as before.

Inversely, I asked myself what influence does light exert upon an action due to electricity. In order to ascertain this, I placed a voltameter‡ in a voltaic circuit of ten pairs of the pile above described, and I estimated the quantities of gas obtained in equal times measured by an excellent seconds pendulum by Ferdinand Berthoud. These quantities were invariable, whether the instrument was in the most complete darkness or exposed to a large beam of light reflected by the heliostat, or lastly, when it was placed in the different rays, coloured or dark, of a spectrum, obtained by projecting a ray of light

* This constant battery is constructed on Daniell's plan. It is formed of cylindrical pairs, the common height of which is $0^{m}\cdot149$, and their diameter for the coppers $0^{m}\cdot06$, and for the zincs $0^{m}\cdot045$. The coppers plunge into (*bocaux*) wide-mouthed glass bottles filled with a concentrated solution of sulphate of copper; the zincs, amalgamated hot, are immersed in a solution of chloride of sodium contained in animal membranes. The pairs communicate with each other by means of very thick copper wires which are soldered to them on one side, and which dip on the other into reservoirs full of mercury, adapted to the edges of the case in which the wide-mouthed bottles are placed. This pile readily preserves in a state of incandescence an iron wire $0^{m}\cdot0005$ in diameter, and more than $0^{m}\cdot3$ in length; by the contact of charcoal cones it gives a luminous point, the brightness of which the eye cannot support, volatilizes between them a thin brass wire, and disengages from 75 to 80 cubic centimetres of gas in a minute by decomposing acidulated water.

† This experiment was made on the 29th of May, 1841, with the assistance of my friend Professor Secretan-Mercier, who was so kind as to lend me his instrument and his skill in using it. It is therefore anterior to the communications of M. Arago on the accelerating processes of M. Daguerre, inserted in the *Comptes Rendus* for the 28th of June and the 5th of July.

‡ This voltameter is formed of two laminæ of platina $0^{m}\cdot03$ in length by $0^{m}\cdot01$ broad, communicating with two wires of pure copper annealed $0^{m}\cdot003$ in diameter, and $0^{m}\cdot441$ long. The electrolyte was a mixture of seven parts pure water with one of concentrated sulphuric acid. The gases were collected over water in a graduated test-tube.

into the camera by a prism of flint glass cut by Fraunhofer himself. These experiments, repeated at different intervals and continued for a sufficient time, have all given the same negative results.

For greater certainty I made the trial in another manner, employing electricity of tension. A tube of thick glass, $0^{m}\cdot015$ in diameter, and $0^{m}\cdot07$ in length, was fixed vertically in a box blackened within and without. A stopper above was traversed by a copper wire of $0^{m}\cdot001$ diameter, communicating metallically with a very thick rod of brass passing through the roof of the box and terminating on the outside by a ball of the same metal $0^{m}\cdot03$ diameter. Another stopper, closing the tube below, admitted a similar wire, which passed out at one angle of the case and extended thence to the ground by means of a chain. The interval between the two wires was made to vary from 1 to 6 millimetres, according to circumstances. The tube was filled in succession with alcohol, oil of turpentine, carburet of sulphur and olive oil, either pure, or mixed in different proportions. A lateral door serving to shut the box was pierced with a vertical slit $0^{m}\cdot003$ wide by $0^{m}\cdot02$ high, the centre of which corresponded to the interval between the wires in the tube. Finally, in the same horizontal plane, the fixed partition had been pierced opposite to a little circular opening which was shut by an ivory screw.

By making sparks of greater or less strength pass through this apparatus, we found that the greatest distance at which the ball should be placed from the conductor for the maximum effect, depended on the nature of the liquid to be decomposed, on the interval between the wire conductors*, and on the energy of the machine. In each experiment, when the proper distance had been attained, it remained invariable, whether the decomposition took place in the thickest darkness, or in the bright light of the sun, or whether the liquid was illuminated through the slit, by colouring it with different tints, by means of coloured glasses, or by projecting upon it the varied tints of the spectrum. The small hole opposite to the slit served to examine the spark, to become assured of its passage and of its decomposing energy.

I remarked that the same number of turns of the plate machine were necessary in order that the tension of the accumulated electricity should be sufficient to make the spark dart upon the ball placed at four or five centimetres distance, whatever was the condition of the light in the experiment.

* The experiment also indicates that this decomposition proceeds successfully only when the extremities of the wires are pointed, and not terminated by balls.

Besides, as the friction of the plate disengages variable quantities of electricity, which also go on decreasing, it was necessary to be independent of this cause of error. With this view I made use of a good Leyden jar, the interior armature of which communicated with the machine, and the exterior with the lower wire of the tube. Here again the spark darted from the great ball of the jar to that of the conductor interrupted in the liquid of the tube, to a distance independent of the colour of the glass which closed the opening of the case (but less than that to which it passed by the mere discharge of the jar itself).

From the preceding researches, may we not conclude that when light or electricity produces a chemical action, the latter is by no means modified by the presence of the other fluid, whatever may be its quality and quantity?

My results entirely agree with those of M. de Haldat *, who found a complete nullity of influence of the electricity which flows off thin wires on the phænomena of diffraction produced by the light passing the edge of these wires. They also recall the observation of Mr. Faraday †, according to which the state of tension produced by the passage of the current from a strong pile through certain electrolytes, such as some aqueous solutions of sulphate of soda and nitrate of lead, or such as melted borate of lead, has no action on a ray of polarized light traversing these liquids, either obliquely or parallel to the direction of the current.

2. *Experiments to show that Electricity does not contain heat* ‡.

Does the electricity of tension contain heat, or are the thermal effects which it causes only to be attributed to the resistance of the conductors through which it passes? This problem, interesting in itself and in its applications, has been resolved by Dr. P. Riess in his beautiful researches on the heating properties of the discharge of the battery §. Yet this solution is indirect and has not been the object of particular experiments; it is deduced from the two following laws:—1st, the quantity of heat set at liberty in a wire by a given electric discharge is in direct proportion to the length and inverse to the diameter of this wire; 2nd, it depends on the nature of the metal forming the wire. From this it follows, virtually, that the discharge will not heat a wire of such dimensions,

* *Ann. de Chimie et de Physique*, t. xli. p. 424.

† Experimental Researches, §§ 951 to 955.

‡ From the *Archives de l'Electricité*.

§ Pogg. *Ann.*, t. xl. p. 432; xliii. p. 47; xlv. p. 1. *Repertorium der Physik*, t. vi. p. 191 (1842).

whatever may be its nature, that it opposes no obstacle to the flowing off of the fluid.

The study of the cooling of electrified bodies (of which I shall shortly make known the results) gave me an opportunity of seeking a direct reply to the question proposed. For this purpose I used a thermo-electric pile making part of Melloni's apparatus, and consisting of bars of bismuth and antimony, metals which have not engaged the attention of Dr. Riess.

After having taken several precautions necessary for preventing any foreign radiation from disturbing the results, by the aid of a powerful electrical machine I caused a series of sparks to pass from one face to the other. The needle of an excellent rheometer by Gourjon, which closed the circuit, was turned briskly, sometimes to the right sometimes to the left of zero, whither it returned rapidly as soon as the production of electricity ceased. Its movements then were not due to the heat which the electricity might have produced*. They were still visible when the circuit was open and when the rheometer, insulated by a thick plate of glass, was connected only with one pole of the pile. Lastly, the same effects were produced when the spark was compelled to traverse the pile from one pole to the other.

It might be objected that the deviations of the needle were produced by a derived or induced current, or by electrical attractions and repulsions. To remove all doubt as to this, I discharged a Leyden jar of moderate dimensions across the pile from one pole to the other, and immediately closed the circuit by plunging the conductors into the apertures of the socket of the rheometer. Avoiding every heating agency (such as the contact of the fingers, &c.), at the points of meeting of the copper wires with their terminal stems of brass, I never perceived the slightest deviation. The experiment has however been repeated many times both with the pile of Melloni's apparatus and with a great thermo-electric pile of thirty-six elements of bismuth and antimony intended for obtaining the spark on mercury.

Lastly, in order that the proof might be conclusive, it was necessary to be certain that the result was not due to a perfect equality in the heating which the discharge had produced at each similar and dissimilar soldering. For this purpose I insulated upon a glass stand a thermo-electric element, having

* It is well known that Prof. D. Colladon obtained as long ago as 1826 a deviation of the needle of the rheometer by the aid of the electricity of tension, either of a machine or of a battery. See *Ann. de Chim. et de Phys.* t. xxxiii. p. 62.

the form of a straight prism with a square base $0^{m}{\cdot}14$ long by $0^{m}{\cdot}01$ in width, formed of one-half bismuth and one-half antimony. Two little hollows made near its extremities were filled with mercury, in order to secure perfect contact with the rheometric wire. The deviation of the needle remained null, even after the thundering discharge of a battery of eight great jars, charged by 125 turns of a machine the plate of which is nearly a metre in diameter, and which usually gives sparks at six centimetres. Now here there was but one soldering, and the apparatus was so sensible that the contact of the finger during two or three seconds projected the needle to 90°.

Electricity then is not hot of itself; its thermic effects proceed only from the obstruction which the conductors oppose to its passage. This conclusion, which appears very natural to me, is interesting on account of the assimilation which may be made by its means with the results to which the study of the diathermancy of voltaic couples leads us. The thermo-electric element employed is the very same as that by the aid of which I repeat in my lectures the experiment of cold produced at the soldering of two metals, fractured by the passage of a voltaic current *. As in all other physical actions, we here perceive the influence of time. An electrical discharge, be it ever so powerful, does not give heat, because it is instantaneous; a current on the contrary produces an elevation or depression of temperature, because it is continued, and its duration allows and produces changes in the statical condition of the molecules of the heterogeneous conductor at the surface of the soldering.

We arrive at the same conclusion with an air-thermometer, the glass bulb of which is at the same time very thin and of great dimensions, and its tube capillary. If sparks from the machine or discharges of a jar are made to fall upon the bulb, no depression of the liquid column is observed, whether the glass of the bulb be naked † or covered with a conducting armature, such as tinfoil. But when the bulb is covered with lamp-black or powdered resin, a heating is perceptible, due to the insulating property and to the combustion of the clothing substance.

Lausanne, Nov. 30, 1842.

* *Arch. de l'Electr.*, t. i. p. 74. *Mém. de la Soc. de Phys. et d'Hist. Nat. de Génève*, t. ix.

† I attribute the slight rise which sometimes takes place to the cold produced by the evaporation, under the electric influence, of the pellicle of vapour adhering to the glass.

3. *On the Cooling of Electrified Bodies**.

It is perhaps impossible to isolate the effects of any one of the imponderable fluids in an absolute manner, and in particular those of electricity and of heat. It is interesting to search out the relations which connect these two universal agents, as well in order that our speculations on their nature may be rendered more conformable to experimental truth, and on account of the part that each of them tends to play in the œconomical applications of the other.

In some recent researches I endeavoured to prove that electricity is not of itself hot†. On the basis of this fact I now purpose to prove that the velocity with which a body cools is independent of the state of electric tension of its surface or of the surrounding matter, all other circumstances being constant. Let us first examine non-porous bodies. A cylindrical vessel of polished tin-plate, supported in a horizontal position by a strong pillar of glass varnished with gum-lac, was filled with boiling water. The orifice was immediately closed with wadding, surrounding a thermometer with a long cylindrical reservoir and entirely mounted in glass. The descent of the mercury might be observed by means of a telescope placed at the distance of ten feet, and the time taken for each degree of cooling reckoned by means of a good seconds pendulum by Ferdinand Berthoud.

Numerous experiments have been made under circumstances of atmospheric pressure, of temperature and of humidity, very nearly identical; care was taken to eliminate the effects of the heating by conduction or by radiation from the supports and communicating rods. These experiments lead to a remarkable equality of the time occupied by the refrigeration of the metallic vessel to the amount of several centigrade degrees, whether or not the vessel were placed in relation with the conductor of an electrical machine, the plate of which was $0^{m}\cdot87$ in diameter, and which, making one turn in a second, sustains the moveable branch of a pith-ball electroscope at an angle of 145° with the vertical. Here are the results of three series of trials:—

Height of the barometer	$0^{m}\cdot7201$
Interior temperature	$+20^{\circ}\cdot4^{c}$
Exterior (Hair) hygrometer	80°

* Read before the Helvetian Society of Natural Sciences, sitting at Lausanne, 26th July, 1843; and now communicated by the Author.

† Archives of Electricity, vol. i. p. 603 [see *antè*, p. 257].

Numbers of the order of the Series.	Mean time of cooling 1° C. of the Electrified Surface.	Non-electrified Surface.
1	113″·43	101″·28
2	100 ·50	97 ·25
3	95 ·00	104 ·25
Mean	102 ·97	100 ·92

Series 1, 2, 3 {electrified surf. . . 102·97; non-electrified surf. 100·92} difference + 2″·05

Series 2 and 3 {electrified surf. . . 97·75; non-electrified surf. 100·75} difference − 3·00

Series 1 and 3 {electrified surf. . . 104·22; non-electrified surf. 102·77} difference + 1·45

Definitive difference + 0″·50.

I wished to control this result by another process, by substituting a secondary for a direct electric tension. A copper vessel supported by a light tripod of wood was filled with olive oil, a Leyden jar was placed in the centre of the liquid, the exterior coating of which was in contact with the vessel and the interior connected with the machine. A rod of brass terminating on one side by a chain which descended upon the ground, on the other side presented a ball a little distance from the copper vessel in order to allow this complex apparatus to become charged. The machine was put in motion in such a manner that the deviation of the electroscope indicated a constant tension. Two series of observations gave 9 for 27° of cooling.

Height of barometer	. .	0m·7218
Interior temperature	. .	+16° 8c
Exterior hygrometer	. .	82°

Numbers of the order of the Series.	Mean time of cooling 1° C. of the Electrified Surface.	Non-electrified Surface.
1	28″·70	27″·96
2	26 ·48	26 ·33
Mean . . .	27 ·590	27 ·145

Definitive difference + 0″·445.

Lastly, I wished to examine the case of porous bodies. The method which I adopted as the most simple and the least incorrect, after various trials, consists in placing some hollow cylinders of wood, such as oak and poplar, in the centre of a thin metallic grating, (that of a calorimeter of Lavoisier and Laplace) but wider by three quarters of an inch, placed on the wooden tripod near the machine. The cylinders were made of a single piece without varnish and provided with a

cover pierced at its centre with an opening destined to receive the thermometer. Five of these apparatus were filled with boiling water and submitted to examination. Here is a result, as an example, for 15° of cooling:—

Nature of the Wood.	Barom.		Inter. Temp.	Exter. humid.	Mean time of cooling 1° C. of the Electrified Surface.	Non-elect. Surface.
Oak .	0m·7113	+	19°·1	100°	71″·60	69″·27
Poplar	0 ·7174	+	19 ·0	90	66 ·86	67 ·73
				Mean . . .	69 ·23	68 ·50

Definitive difference + 0″·73.

I attribute the coincidence of sign of the definitive differences to a fortuitous circumstance which would disappear by combining a greater number of series, although it is in favour of the duration of cooling of the electrified surface, in the examples already mentioned. Moreover, these differences are of so slight a kind that they may be reckoned amongst the possible, nay, I may say probable, errors of observation.

This nullity of influence of the electro-static state of the porous or metallic *parietes* by which a calorific radiation is brought about at the time of its cooling, reminds us of the reciprocal indifference of electricity and of light when one of the two fluids produce a chemical action *. It tends to a conclusion contrary to the opinion of some physiologists, that the electric state, whether of the human body or of the atmosphere, has no influence on the loss of animal heat in a given time, and consequently none on the œconomy of the general state of health, nor on the functions of respiration and of digestion, which are perhaps the only sources of this heat †. In my experiments on organic parietes there has never been any exudation of liquid on the exterior; a change in the chemical nature, and therefore in the temperature of this liquid, is not then to be expected; nor must we look for phænomena of evaporation and of cooling, still less for internal lesions, the probable or certain existence of which had been alleged in more than one case by a skilful physicist ‡.

* Archives of Electricity, vol. ii. p. 596. [*antè*, p. 254.]

† See the remarks of M. Dumas on M. Dulong's researches on Animal Heat, and on the correction to be made of the coefficient of the calorific power of hydrogen.—*Annales de Chimie et de Physique*, 3me Sér. t. viii. p. 180. (June 1843.)

‡ Peltier's memoir on different kinds of fogs, SS. 28–30. *Mém. de l'Acad. de Bruxelles*, t.xv.; *Annales de Chimie et de Physique*, 3me Sér. t. vi. p. 129. (Oct. 1842.)

XXXII. *On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat.* By J. P. Joule, *Esq.**

IT is pretty generally, I believe, taken for granted that the electric forces which are put into play by the magneto-electrical machine, possess, throughout the whole circuit, the same calorific properties as currents arising from other sources. And indeed when we consider heat not as a *substance*, but as a *state of vibration*, there appears to be no reason why it should not be induced by an action of a simply mechanical character, such, for instance, as is presented in the revolution of a coil of wire before the poles of a permanent magnet. At the same time it must be admitted that hitherto no experiments have been made decisive of this very interesting question; for all of them refer to a particular part of the circuit only, leaving it a matter of doubt whether the heat observed was *generated*, or merely *transferred from the coils* in which the magneto-electricity was induced, the coils themselves becoming cold. The latter view did not seem to me very improbable, considering the facts which I had already succeeded in proving, viz. that the heat evolved by the voltaic battery is *definite*† for the chemical changes taking place at the same time; and that the heat rendered "latent" in the electrolysis of water is at the expense of the heat which would otherwise have been evolved in a free state by the circuit ‡—facts which, among others, seem to prove that *arrangement* only, not *generation* of heat, takes place in the voltaic apparatus; the simply conducting parts of the circuit evolving that which was previously latent in the battery. And Peltier, by his discovery that cold is produced by a current passing from bismuth to antimony, had, I conceived, proved to a great extent that the heat evolved by thermo-electricity is transferred§ from the heated solder, no heat being *generated*. I resolved therefore to endeavour to clear up the uncertainty with respect to magneto-electrical heat. In this attempt I have met with results which will I hope be worthy the attention of the British Association.

* Read before the Section of Mathematical and Physical Science of the British Association, meeting at Cork on the 21st of August 1843; and now communicated by the Author.

† Phil. Mag. S. 3. vol. xix. p. 275.

‡ Memoirs of the Literary and Philosophical Society of Manchester, 2nd series, vol. vii. (part 2.) p. 97.

§ The quantity of heat thus transferred is, I doubt not, proportional to the square of the difference between the temperatures of the two solders. I have attempted an experimental demonstration of this law, but owing to the extreme minuteness of the quantities of heat in question, I have not been able to arrive at any satisfactory result.

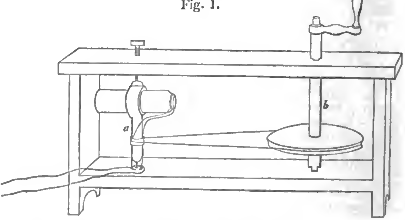
Part I.—*On the Calorific Effects of Magneto-Electricity.*

The general plan which I proposed to adopt in my experiments under this head, was to revolve a small compound electro-magnet, immersed in a glass vessel containing water, between the poles of a powerful magnet; to measure the electricity thence arising by an accurate galvanometer; and to ascertain the calorific effect of the coil of the electro-magnet by the change of temperature in the water surrounding it.

The revolving electro-magnet was constructed in the following manner:—Six plates of annealed hoop-iron, each eight inches long, $1\frac{1}{8}$ inch broad, and $\frac{1}{16}$th of an inch thick, were insulated from each other by slips of oiled paper, and then bound tightly together by a ribbon of oiled silk. Twenty-one yards of copper wire $\frac{1}{18}$th of an inch thick, well covered with silk, were wound on the bundle of insulated iron plates, from one end of it to the other and back again, so that both of the terminals were at the same end.

Having next provided a glass tube sealed at one end, the length of which was $8\frac{3}{4}$ inches, the exterior diameter 2·33 inches, and the thickness 0·2 of an inch, I fastened it in a round hole, cut out of the centre of the wooden revolving piece *a*, fig. 1. The glass was then covered with tinfoil, excepting a

Fig. 1.



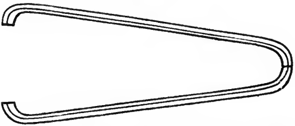
narrow slip in the direction of its length, which was left in order to interrupt magneto-electrical currents in the tinfoil during the experiments. Over the tinfoil small cylindrical sticks of wood were placed at intervals of about an inch, and over these again a strip of flannel was tightly bound, so as to

inclose a stratum of air between it and the tinfoil. Lastly, the flannel was well varnished. By these precautions the injurious effects of radiation, and especially of convection of heat in consequence of the impact of air at great velocities of rotation, were obviated to a great extent.

The small compound electro-magnet was now put into the tube, and the terminals of its wire, tipped with platinum, were arranged so as to dip into the mercury of a commutator*, consisting of two semicircular grooves cut out of the base of the frame, fig. 1. By means of wires connected with the mercury of the commutator, I could connect the revolving electro-magnet with a galvanometer or any other apparatus.

In the first experiments I employed two electro-magnets (formerly belonging to an electro-magnetic engine) for the purpose of inducing the magneto-electricity. They were situated with two of their poles on opposite sides of the revolving electro-magnet, and the other two joining each other beneath the frame. I have drawn fig. 2 representing these

Fig. 2.



electro magnets by themselves, to prevent confusing fig. 1. The iron of which they were made was one yard six inches long, three inches broad, and half an inch thick. The wire which was wound upon them was $\frac{1}{20}$th of an inch thick; it was arranged so as to form a sixfold conductor a hundred yards long.

The following is the method in which my experiments were made:—Having removed the revolving piece from its place (which is done with great facility by lifting the top of the frame, and with it the brass socket in which the upper steel pivot of the revolving piece works), I filled the tube containing the small compound electro-magnet with $9\frac{3}{4}$ oz. of water. After

* I had made previous experiments in order to ascertain the best form of commutator, but found none to answer my purpose as well as the above. I found an advantage in covering the mercury with a little water. The steadiness of the needle of the galvanometer during the experiments proved the efficacy of this arrangement.

stirring the water until the heat was equably diffused, its temperature was ascertained by a very delicate thermometer, by which I could estimate a change of temperature equal to about $\frac{1}{50}$th of Fahrenheit's degree. A cork covered with several folds of greased paper was then forced into the mouth of the tube, and kept in its place by a wire passing over the whole, and tightened by means of one or two small wooden wedges. The revolving piece was then restored to its place as quickly as possible, and revolved between the poles of the large electro-magnets for a quarter of an hour, during which time the deflections of the galvanometer and the temperature of the room were carefully noted. Finally, another observation with the thermometer detected any change that had taken place in the temperature of the water.

Notwithstanding the precautions taken against the injurious effects of radiation and convection of heat, I was led into error by my first trials: the water had lost heat, even when the temperature of the room was such as led me to anticipate a contrary result. I did not stop to inquire into the cause of the anomaly, but I provided effectually against its interference with the subsequent results by interpolating the experiments with others made under the same circumstances, except as regards the communication of the battery with the stationary electro-magnets, which was in these instances broken. And to avoid any objection which might be made with regard to the heat, however trifling, evolved by the wires of the large electro-magnets, the thermometer employed in registering the temperature of the air was situated so as to receive the influence arising from that source equally with the revolving piece.

I will now give a series of experiments in which six Daniell's cells, each 25 inches high and $5\frac{1}{2}$ inches in diameter, were alternately connected and disconnected with the large stationary electro-magnets. The galvanometer, connected through the commutator with the revolving electro-magnet, had a coil of a foot in diameter, consisting of five turns of copper wire, and a needle six inches long. Its deflections could be turned into quantities of current by means of a table constructed from previous experiments. The galvanometer was situated so as to be out of the reach of the attractions of the large electro-magnets, and every other precaution was taken to render the experiments worthy of reliance. The rotation was in every instance carried on for exactly a quarter of an hour.

Series No. 1.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 5 turns.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
April 15, P.M.	Battery contact broken.	600	0° 0'	54·69°	0·19°+	54·90°	54·85°	0·05° loss
	Battery in connexion.	600	21 0	54·67	0·20+	54·85	54·88	0·03 gain
	Battery contact broken.	600	0 0	54·61	0·24+	54·88	54·83	0·05 loss
	Battery in connexion.	600	24 0	54·65	0·23+	54·85	54·92	0·07 gain
	Mean, Battery in connexion.	600	22 30	...	0·21+	...	...	0·05 gain
	Mean, Battery contact broken.	600	0 0	...	0·21+	...	...	0·05 loss
	Corrected Result.	600 22° 30' = 0·177* of cur. mag.-elect.						0·10 gain

Having thus detected the evolution of heat from the coil of the magneto-electrical apparatus, my next business was to confirm the fact by exposing the revolving electro-magnet to a more powerful magnetic influence; and to do so with the greater convenience, I determined on the construction of a new stationary electro-magnet, by which I might obtain a more advantageous employment of the electricity of the battery. Availing myself of previous experience, I succeeded in producing an electro-magnet possessing greater power of attraction from a distance than any other I believe on record. On this account a description of it in greater detail than is absolutely necessary to the subject of this paper will not I hope be deemed superfluous.

A piece of half-inch boiler plate iron was cut into the shape

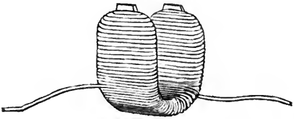
Fig. 3.

* Throughout the paper I have called that quantity of current *unity*, which, passing equably for an hour of time, can decompose a chemical equivalent expressed in grains.

represented by fig. 3. Its length was thirty-two inches; its breadth in the middle part eight inches; at the ends three inches. It was bent nearly into the shape of the letter U, so that the shortest distance between the poles was slightly more than ten inches.

Twenty-two strands of copper wire, each 106 yards long and about one-twentieth of an inch in diameter, were now bound tightly together with tape. The insulated bundle of wires, weighing more than sixty pounds, was then wrapped upon the iron, which had itself been previously insulated by a fold of calico. Fig. 4 represents, in perspective, the electro-magnet in its completed state.

Fig. 4.



In arranging the voltaic battery for its excitation, care was taken to render the resistance to conduction of the battery equal, as nearly as possible, to that of the coil, Prof. Jacobi having proved that to be the most advantageous arrangement. Ten of my large Daniell's cells, arranged in a series of five double pairs, fulfilled this condition very well, producing a magnetic energy in the iron superior to anything I had previously witnessed. I will mention the results of a few experiments in order to give some definite idea of it.

1st. The force with which a bar of iron three inches broad and half an inch thick was attracted to the poles, was equal, at the distance of $\frac{1}{16}$th of an inch, to 100 lbs; at $\frac{1}{4}$th of an inch to 30 lbs; at half an inch to $10\frac{1}{2}$ lbs.; and at one inch to 4 lbs. 13 oz.* 2nd. A small rod of iron three inches long, weighing 148 grs., held vertically under one of the poles, would jump through an interval of $1\frac{3}{4}$ inch; a needle three

* The above electro-magnet being constructed for a specific purpose, was not adapted for displaying itself to the best advantage in these instances. On account of the extension of its poles (three inches by half an inch) many of the lines of magnetic attraction were necessarily in very oblique directions. Theoretically, circular poles should give the greatest attraction from small distances.

inches long, weighing 4 grains, would jump from a distance of $3\frac{1}{2}$ inches.

Having fixed the electro-magnet just described with its poles upwards, and on opposite sides of the revolving electro-magnet, I arranged to it the battery of ten cells, in a series of five double pairs, and, experimenting as before, I obtained a second series of results. The galvanometer used in the present instance was in every respect similar to that previously described, with the exception of the coil, which now consisted of a single turn of thick copper wire. Great care was taken to prevent, by its distance from, and relative position with the electro-magnet, any interference of the latter with its indications.

No. 2.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of one turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
May 6, A.M.	Battery in connexion.	600	22° 0'	58°·93	0°·17+	58°·20	60°·00	1°·80 gain
	Battery contact broken.	600	0 0	59·60	0·40+	60·02	59·98	0·04 loss
	Battery in connexion.	600	24 0	59·55	1·23+	59·90	61·67	1·77 gain
May 6, P.M.	Battery contact broken.	600	0 0	59·45	0·19+	59·78	59·50	0·28 loss
	Battery in connexion.	600	24 45	58·30	0·05+	57·35	59·35	2·00 gain
May 8, A.M.	Battery in connexion.	600	22 0	57·74	0·32+	57·28	58·83	1·55 gain
	Battery contact broken.	600	0 0	58·35	0·49+	58·83	58·85	0·02 gain
	Battery in connexion.	600	21 20	58·73	0·78+	58·83	60·20	1·37 gain
	Mean, Battery in connexion.	600	22 49	...	0·51+	...	...	1·70 gain
	Mean, Battery contact broken.	600	0 0	...	0·36+	...	...	0·10 loss
	Corrected Result.	600	22° 49' = 0·902 of cur. mag.-elect.					1·84 gain

The corrected result is obtained as before, by adding the loss sustained when contact with the battery was broken, to the heat gained when the battery was in connexion. I have in the present instance, however, made a further correction of

0°·04 on account of the difference between the *mean differences* 0°·51 and 0°·36. The ground of this correction is the result of a previous experiment, in which, by revolving the apparatus at 94° in an atmosphere of 60°, the water sustained a loss of 7°·6, or about one quarter of the difference between the temperature of the atmosphere and the mean temperature of the water.

With the same electro-magnet, but using a battery of only four cells, arranged in a series of two double pairs, by which I expected to obtain about half as much magnetism in the iron, the following results were obtained:—

No. 3.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 5 turns.		Mean Temperature of Room.	Mean Difference.	Temperature of Water.		Loss or Gain.
							Before.	After.	
May 8, P.M.	Battery in connexion.	600	38°	0′	57°·00	0°·02—	56°·73	57°·23	0°·50 gain
	Battery contact broken.	600	0	0	57·25	0·0	57·23	57·27	0·04 gain
	Battery in connexion.	600	38	30	57·53	0·09+	57·35	57·90	0·55 gain
May 9, P.M.	Battery in connexion.	600	39	45	56·37	0·45—	55·60	56·25	0·65 gain
	Battery contact broken.	600	0	0	56·75	0·39—	56·27	56·45	0·18 gain
	Battery in connexion.	600	38	45	57·14	0·37—	56·50	57·05	0·55 gain
	Mean, Battery in connexion.	600	38	45	...	0·19—	...	...	0·56 gain
	Mean, Battery contact broken.	600	0	0	...	0·19—	...	...	0·11 gain
	Corrected Result.	600 38° 45′ = 0·418 of cur. mag.-elect.							0·45 gain

In the next experiments a battery of ten cells in a series of five double pairs was used for the purpose of exciting the large stationary electro-magnet. But, dismissing the galvanometer and the other extra parts of the circuit, I connected the terminal wires of the electro-magnet together, so as to obtain the whole effect of the magneto-electricity. The resistance of the coil of the revolving electro-magnet being to that of the whole circuit employed in the experiments No. 2 as 1 : 1·13, and 0·902 of current being obtained in those experiments, I ex-

pected to obtain the calorific effect of 1·019 in the new series.

No. 4.

		Revolutions of Electro-Magnet per minute.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	Temperature of Water. After.	Loss or Gain.
May 10, P.M.	Battery in connexion.	600	56°·85	0°·61—	54°·98	57°·50	2°·52 gain
	Battery contact broken.	600	57·37	0·12+	57·48	57·50	0·02 gain
	Battery in connexion.	600	57·52	1·08+	57·48	59·73	2·25 gain
	Mean, Battery in connexion.	600	...	0·23+	...	...	2·38 gain
	Mean, Battery contact broken.	600	...	0·12+	...	...	0·02 gain
	Corrected Result.	600	1·019 of cur. mag.-elect.				2·39 gain

It seemed to me very desirable to repeat the experiments, substituting *steel* magnets for the stationary electro-magnets hitherto used. With this intention I constructed two magnets, each consisting of a number of thin plates of hard steel, —an arrangement which we owe to Dr. Scoresby. My metal was, unfortunately, not of very good quality, but nevertheless an attractive force was obtained sufficiently powerful to overcome the gravity of a small key weighing 47 grs., placed at the distance of three-eighths of an inch. The following results were obtained by revolving the small compound electro-magnet between the poles of the steel magnets.

In order to obtain the whole calorific effect of the steel magnets, I now, as in Series No. 4, connected the terminal wires of the revolving electro-magnet, and interpolated the experiments with others in which that connexion was broken. The resistance of the coil of the revolving electro-magnet being to the resistance of the whole circuit used in the experiments marked No. 5 as 1 : 1·44, and 0·236 of current electricity being obtained in those experiments, I expected to obtain in the present series the calorific effect of 0·34 of current magneto-electricity.

No. 5.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 5 turns.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
May 16, A.M.	Circuit complete.	600	26° 0′	59°·72	0°·0	59°·73	59°·70	0°·03 loss
	Circuit broken.	600	0 0	59·82	0·20—	59·70	59·55	0·15 loss
	Circuit complete.	600	29 0	59·95	0·41—	59·55	59·53	0·02 loss
	Circuit broken.	600	0 0	59·58	0·12—	59·52	59·40	0·12 loss
	Circuit complete.	600	27 0	59·65	0·25—	59·40	59·40	0
	Mean, Circuit complete.	600	27 20	...	0·22—	...	...	0·016 loss
	Mean, Circuit broken.	600	0 0	...	0·16—	...	...	0·135 loss
	Corrected Result.	600	27° 20′ = 0·236 of cur. mag.-elect.					0·10 gain

No. 6.

		Revolutions of Electro-Magnet per minute.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
May 17, A.M.	Terminals joined.	600	59°·07	0°·20—	58°·82	58°·92	0°·10 gain
	Terminals separated.	600	59·07	0·22—	58·92	58·78	0·14 loss
	Terminals joined.	600	58·96	0·20—	58·75	58·78	0·03 gain
	Terminals separated.	600	58·88	0·18—	58·78	58·63	0·15 loss
	Mean, Terminals joined.	600	...	0·20—	...	...	0·065 gain
	Mean, Terminals separated.	600	...	0·20—	...	...	0·145 loss
	Corrected Result.	600	0·34 of cur. mag.-elect.				0·21 gain

Although any considerable development of electrical currents in the iron of the revolving electro-magnet was prevented by its disposition in a number of thin plates insulated from each other, I apprehended that they might, under a powerful inductive influence, exist separately in each plate to such an extent as to produce an appreciable quantity of heat. To ascertain the fact, the terminals of the wire of the revolving electro-magnet were insulated from each other, while the latter was subjected to the inductive influence of the large electro-magnet excited by ten cells in a series of five double pairs. The experiments were interpolated with others in which contact with the battery was broken. As we shall hereafter give in detail experiments of the same class, it will not be necessary to do more at present than to state that the mean result of the present series, consisting of eight trials, gave $0^{\circ}{\cdot}28$ as the quantity of heat evolved by the iron alone.

We are now able to collect the results of the preceding experiments so as to discover the laws by which the development of the heat is regulated. The fourth column of the following table, containing the heat due to the currents circulating in the iron alone, is constructed on the basis of a law which we shall subsequently prove, viz. *the heat evolved by a bar of iron revolving between the poles of a magnet is proportional to the square of the inductive force.* Column 5 gives the heat evolved by the coils of the electro-magnet alone. No elimination is required for the results of series Nos. 5 and 6, because in them the iron of the revolving electro-magnet was subject to the influence of the steel magnets in the interpolating, as well as in the other experiments.

Table I.

Series of Experiments.	Current Magneto-Electricity.	Heat actually evolved.	Correction for Currents in the Iron.	Corrected Heat.	Squares of Numbers proportional to those in column 2.	Heat due to Voltaic Currents of the intensities given in col. 2.	The Numbers of column 7 multiplied by $\frac{4}{3}$.
No. 1.	0·177	$0^{\circ}{\cdot}10$	$0^{\circ}{\cdot}02$	$0^{\circ}{\cdot}08$	$0^{\circ}{\cdot}062$	$0^{\circ}{\cdot}040$	$0^{\circ}{\cdot}053$
No. 2.	0·902	1·84	0·28	1·56	1·614	1·040	1·386
No. 3.	0·418	0·45	0·09	0·36	0·346	0·224	0·299
No. 4.	1·019	2·39	0·28	2·11	2·060	1·327	1·769
No. 5.	0·236	0·10	0	0·10	0·109	0·071	0·091
No. 6.	0·340	0·21	0	0·21	0·229	0·148	0·197
1.	2.	3.	4.	5.	6.	7.	8.

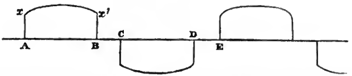
On comparing the corrected results in column 5 with the squares of magneto-electricity given in column 6, it will be abundantly manifest that *the heat evolved by the coil of the magneto-electrical machine is proportional* (*cæteris paribus*) *to the square of the current.*

Column 7, containing the heat due to voltaic currents of the quantities stated in column 2, is constructed on the basis of three very careful experiments on the heat evolved by passing currents through the coil of the small compound electro-magnet. I observed an increase in the temperature of the water equal to 5°·3, 5°·46, and 5°·9 respectively, when 2·028, 2·078, and 2·145 of current voltaic electricity were passed, each during a quarter of an hour, through the coil. Reducing the first and second experiments to the electricity of the third according to the squares of the current, we have 5°·93, 5°·82 and 5°·9 for 2·145 of current. The mean of these is 5°·88, a datum from which the theoretical results of the preceding and subsequent tables are calculated.

But in comparing the heat evolved by magneto-, with that evolved by voltaic electricity, we must remember that the former is propagated by pulsations, the latter uniformly. Now since the square of the mean of unequal numbers is always less than the mean of their squares, it is obvious that the magnetic effect at the galvanometer will bear a greater proportion to the heat evolved by the voltaic, than the magneto-electricity; so that it is impossible to institute a strict comparison without ascertaining previously the intensity of the magneto-electricity at every instant of the revolution of the revolving electro-magnet. I have not been able to devise any very accurate means for attaining this object: but judging from the comparative brilliancy of the sparks when the commutator was arranged so as to break contact with the mercury at different positions of the revolving electro-magnet with respect to the poles of the stationary electro-magnet, there appeared to be but little variation in the intensity of the magneto-electricity during $\frac{3}{4}$ of each revolution. The remaining $\frac{1}{4}$ (during which the revolving electro-magnet passes the poles of the stationary electro-magnet) is occupied in the conversion of the direction of the electricity. In the experiments all flow of electricity during this $\frac{1}{4}$ is cut off by the divisions of the commutator. In illustration of this I have drawn fig. 5, in which the direction and intensity of the magneto-electricity are represented by ordinates Ax, &c., perpendicular to the straight line A B C D E; the intermediate spaces B C, C D, &c., represent the time during which the electricity is wholly cut off by the divisions of the commutator.

Were A x x' B, &c. perfect rectangles, it is obvious that the heat due to a given deflection of the galvanometer would be

Fig. 5.



$\frac{4}{3}$ of that due to the same deflection and an uniform current, and column 8 of the table would contain exact theoretical results. But as this is not precisely the case, the numbers in that column are somewhat under the truth.

Bearing this in mind in the comparison of columns 5 and 8, it will, I think, be admitted that the experiments afford decisive evidence that *the heat evolved by the coil of the magneto-electrical machine is governed by the same laws as those which regulate the heat evolved by the voltaic apparatus; and exists also in the same quantity under comparable circumstances.*

Although very little doubt could exist with regard to the heating power of magneto-electricity *beyond* the coil, I thought it would nevertheless be well to follow it there, in order to render the investigation more complete: I am not aware of any previous experiments of the kind.

I immersed five or six yards of insulated copper wire of $\frac{1}{40}$th of an inch diameter in a flask holding about 12 oz. of water. The terminals of the wire were connected on one hand with the galvanometer of five turns and on the other with the commutator, and the circuit was completed by a wire extending from the galvanometer to the other compartment of the commutator. The revolving electro-magnet was now subjected to the inductive influence of the large electro-magnet excited by ten cells in a series of five double pairs, and rotated at the rate of 600 revolutions per minute during a quarter of an hour. The needle of the galvanometer, which remained as usual pretty steady, indicated a mean deflection of $32^\circ\,40' = 0{\cdot}31$ of current: and the heat evolved was found to be $0^\circ{\cdot}46$, after the correction on account of the temperature of the surrounding air had been applied. Another experiment gave me $0^\circ{\cdot}4$ for 0·286. The mean of the two is $0^\circ{\cdot}43$ for 0·298 current magneto-electricity.

By passing a voltaic current from four cells in series through the wire, I found that 2·02 of current flowing uniformly evolved $12^\circ{\cdot}0$ in a quarter of an hour. Reducing this to 0·298 of cur-

rent we have $\left(\frac{0{\cdot}298}{2{\cdot}02}\right)^2 \times 12^\circ = 0^\circ{\cdot}261$. The product of this by $\frac{4}{3}$ (on account of the pulsatory character of the magneto-current) gives $0^\circ{\cdot}348$, which, as theory demands, is somewhat less than the quantity found by experiment.

[To be continued.]

XXXIII. *On the Storms of Tropical Latitudes.* By William Brown, *Jun.*

[Continued from p. 217 and concluded.]

THE storms of the tropics and of temperate regions, though thus referable to the same source, have yet some marked differences. The principal of these are:—the greater violence of the former; the isolation of each individual storm; the less extent of each particular portion of it, and the much greater extent and regularity of its progressive motion; and the more rapid though less depression of the barometer.

It may seem difficult at first to account for the violence of tropical hurricanes, but the difficulty disappears when we cease to compare the force of the upper current with that of the lower which sweeps over the earth, where the friction upon the surface prevents its gaining a great velocity by soon putting a limit to its acceleration. But as at the beginning of the hurricanes which arise in the region of the trade-wind the lower current has not effected the change corresponding to that of the upper one, both therefore flowing in the same direction, the effect of friction must be so slight that we may almost disregard it; as we may do also in other portions of the zone of the tropics, where a permanent reversal of the currents is effected; because it must be supposed that the upper and lower currents of the atmosphere are separated from each other by an interval of calm air; therefore the upper current when confined to the upper strata of the atmosphere, subject as it is from the constant decrease in temperature it meets with to an accelerating force, may attain a very great velocity, more especially in the former regions, before the effect of friction is sufficiently powerful to put a limit to its acceleration.

Thus as it matters little whether the difference of temperature, by which the wind acquires force sufficient to give the velocity of a hurricane notwithstanding the resistance of the air which it must displace, exists between columns of air nearly adjacent, or at the distance of many degrees of latitude, there seems no difficulty in conceiving the difference to be sufficiently great.

The comparative exemption of the upper current from fric-

tion may also be deduced from observation, being shown by its effect on the mean height of the barometer, as already observed in a former paper, though it is probable that amongst the causes there given as combining to produce this effect, due prominence was not given to this one; for we find that the mean height of the barometer at latitude 32° is one-tenth of an inch above that at latitude 22°; in opposition to the effect of the centrifugal force of the earth's rotation. Now as the greater part of this distance is within the zone of the trade-winds, the principal cause must be the excess of the influx of the upper current; the lower one being retarded by friction, to which the former is not exposed. But we have another instance more striking than this. In some comparative observations of the height of the barometer during different seasons, inserted by W. C. Redfield in the American Journal of Science, vol. xxxviii. p. 267, it appears that the mean height of the barometer at Canton is 30·246 inches in winter, and 29·974 inches in summer: now Canton is situated in about latitude 23°, thus much nearer the northern verge of the monsoons than the southern: now the north-east monsoon (or the trade-wind) blows in winter, and the south-west monsoon in summer; thus, as the upper currents are of course the opposite of these, Canton is near the influx of the upper current in winter and near its egress in summer, and hence the barometer stands 0·272 inch higher in the former season than in the latter*.

* The influence of the change in the direction of the currents is even greater than this, for omitting the months during which the monsoons change, the difference of pressure is increased to about one-third of an inch; the amount which it is necessary to reduce the elasticity of the upper current in order to counteract the effect of friction upon the lower.

In strict accordance with this variation are those of high latitudes, which are the opposite of those of the monsoons; for it appears from the observations just quoted, that at Newfoundland, latitude 49°, the summer pressure is 0·145 inch greater than the winter. The effect of friction upon the lower current, and that of the opposition given to it by the falling of the upper one being the same,—to cause in one part an accumulation of air and in another a deficiency, the accumulation will be in polar regions, and the deficiency in high latitudes; and as these currents are of so much greater force in winter than in summer, their influence will of course be greatest in the former season; hence this deficiency of pressure is greatest in high latitude during winter, and the greater pressure of those near the pole ought to be at the same time increased.

The apparent exception to this in the pressure at New York, which according to Redfield is 0·044 inch higher in winter than in summer, is probably due to its vicinity to the northern verge of the region of the trade winds, and the position of the Gulf of Mexico with regard to the continent of America. The mean pressure in this country for the season is thus given by L. Howard:—summer, 29·883; autumn, 29·833; spring, 29·800; winter, 29·778 (Climate of London, vol. i. p. 210.) The mean pressure in

That this is the true explanation of this variation can scarcely, I think, admit of doubt; it requires of course that the pressure of the atmosphere near the southern confines of the monsoons should vary simultaneously with that of the northern, but in the opposite direction, and that at the equator, or midway in the course of each monsoon, it should remain nearly stationary throughout the year; and also that the variation should be general throughout the zone of the tropics, because although the atmospheric currents in only some portions of it have the regular alternations of monsoons, they are everywhere more or less influenced both in force and direction by the movement of the sun in declination. I am not aware of any observations in the longitudes of India or China to enable us to answer the first of these questions with regard to the monsoons themselves, but the occurrence of the variations in the required order in the longitudes of the New World is shown by the observations given by Humboldt in his "Personal Narrative;" the results of which with regard to this subject, he thus states (English edit., vol. vi. p. 747):—"It is interesting to compare the variations of the weight of the atmosphere in the vicinity of the two tropics. At Rio Janeiro (south latitude 23° 30′) the extreme barometric mean of December and August, and at the Havannah (north latitude 22° 30′), that of September and January differs nearly 8 millimetres (0·315 inch), whilst at Bagota (north latitude 4° 30′), nearer the equator, the monthly mean does not swerve $1\frac{1}{2}$ millimetre" (0·059 inch). And in a note to the above, these means are thus given:—"Rio Janeiro; mean height in December, 337·02 lines (thermometer 25·7° Ct.); in August, 340·59 lines (therm. 22·1° Ct.); at the Havannah, in September, 761·23mm (therm. 28·8° Ct.); in January, 768·09mm (therm. 21·1° Ct.). Reduced to the temperature of zero (Cent.), the difference near the tropic of Capricorn is 8·3mm (0·327 inch), near the tropic of Cancer 7·9mm (0·311 inch)."

But if this reasoning accounts for the violence of tropical storms, it also accounts for the less violence of those of high latitudes; because, as the south wind will sustain a loss of force by every descent, and as the north wind in the part of its course near its termination will frequently be an ascending current, and carry with it its motion from north, thereby retarding the progress of the air flowing in the opposite direction; the frequent alternation of these winds upon the surface of the earth in high latitudes, prevents its attaining the force

the regions of "variable winds" is, however, without doubt influenced by the prevalence of particular winds, but that this is not the sole cause of difference is very evident.

of tropical hurricanes, by checking the acceleration of the southern or upper current.

The differences of the two descriptions of storms, with regard to the fall of the barometer and the extent of each particular portion of the hurricane,are well compared by Professor Dove to those between a deep ravine with precipitous sides, and an extensive valley with gentle declivities; a comparison however holding much more truly with regard to the views here taken, than to those adopted by Professor Dove in advocating the hypothesis of a whirlwind*.

It will be easily seen that these differences arise from the difference in the force of the wind. The more violent the wind the more rapid will be the fall of the barometer, but at the same time the resistance to the force of the wind will increase more rapidly also, and overcome it in less time than when the wind is less violent.

The remaining differences are obviously referable to the same origin as those generally existing between the meteorological phænomena of these two divisions of the globe,—the great complexity of the causes which affect the distribution of heat and atmospheric vapour in temperate regions as compared with those of the tropics. Thus whilst in the latter each storm is generally one individual phænomenon, the former are frequently visited by them in rapid succession, each succeeding one appearing before the subsidence of the last, and thus often preventing the regularityof the succession of the currents, here described as belonging to tropical gales and sometimes to these, being observed.

An interesting observation regarding the locality of hurricanes has been made by Col. Reid, which is the fact of the exemption from them of the region adjacent to St. Helena, whose position occurs on the line of least magnetic intensity, as drawn on Major Sabine's chart; not however as Col. Reid appears to suggest, as pointing out the existence of a force not previously recognised as at work in the movements of the atmosphere, but as indicating the connexion between the localities of storms and climate; Sir David Brewster having shown the relation existing between the isodynamic magnetic lines and the isothermal lines; both these series of lines agreeing in general in their deviations from the circles of latitudes, the lines of greatest magnetic intensity following those of greatest heat. The dependence of the force of the wind on the relative difference of temperature of adjacent latitudes, has been before shown with regard to seasons; storms will be

* Dove on the Law of Storms (Taylor's Scientific Memoirs, vol. iii. (part x.) p. 217).

most violent therefore where this is the greatest; and thus arises the great violence of the storms on the American coast, compared with that in general of British storms.

If then I have been successful in my attempt to establish the views set forth in this essay, all the more important phænomena of the wind may be simply classed with those of the southerly and northerly breezes of our own climate, the various directions of the wind arising either from the simple deflection of one of the currents by the rotation of the earth, or from the collision of the two currents after being so deflected; the barometer rising or falling, as the density of the one or the momentum of the other prevails. But there yet remains the ultimate step in this inquiry, to discover the immediate or proximate cause of the descent of the upper current at any particular time.

The principal difficulty to be overcome seems to be, that the descending air is warmer, and therefore lighter than that previously occupying its place; there must therefore exist some cause capable of producing a descent of air in opposition to its density. Such a cause may perhaps be found in the diffusive tendency of aqueous vapour. There seems no reason to deny this property of perfect gases to the vapour existing, as it is now admitted, as an independent constituent of the atmosphere; and as the upper current contains the largest quantity of vapour, there will exist a tendency in the air of the higher and of the lower currents to intermix: according to the conclusions deduced by Thomas Thompson (Phil. Mag. vol. iv. p. 321) from Prof. Graham's law of the diffusive force of gases, this force remains throughout all stages of the intermingling of the gases to be inversely as the square root of their densities. It would seem therefore in the present case to require, in order that the upper air should be made to descend and the lower to ascend, that the quantities of vapour in the two currents (supposing the air of each to be of the same temperature) should be such, as to make the square root of the number expressing the density of the mixture of air and vapour of the lower current relatively to that of the higher, equal to the number expressing the density of the dry air of the same current relatively to that of the higher, at their real temperatures. This however would require too great a difference to be allowed; but if the air itself does not at first descend, the vapour, by infiltrating through the particles of air, and arriving amongst comparatively cold air* will be partially condensed (perhaps the cause of the rain which so

* The difference of temperature arising from the difference of elasticity of air of unequal elevations is here left out of consideration.

frequently follows the change of the wind from north-east to south-east), and the latent heat emitted during the condensation may raise the temperature of the air so as to diminish its density sufficiently to enable the diffusive force of succeeding portions of vapour to carry down some of the air of the upper current, which, by immediately checking the air flowing in the opposite direction by the force of its momentum, will be followed by another quantity of air to supply the want occasioned by the check; and thus when the difference in the quantity of vapour in the opposite currents is sufficient to cause a tolerably abundant flow of the vapour of the upper one into the strata of the lower, the upper current may, by the above process, gradually work its way to the surface of the earth.

Although this hypothesis is offered as little more than a conjecture, observation has afforded me some rather striking instances of a change of wind from north to south immediately following a sudden dryness of the atmosphere; and as rain so frequently occurs at the changing of the wind to south, it may receive some support from the results of some observations, inserted by Prof. Loomis in the American Journal of Science for October 1841, showing the frequent occurrence of rain very shortly after an unusual dryness of the air. It is not, however, proposed to the exclusion of other causes cooperating with it, as for instance an interruption of the lower current by a local elevation or depression of temperature, by which a deficiency of air would somewhere be produced; but that it is frequently opposed in its descent by the lower current, is apparently shown by the wind changing from north-east to south-east, the south wind being deflected by the force of that from north-east.

XXXIV. *On the Changes in Composition of the Milk of a Cow, according to its Exercise and Food.* By LYON PLAYFAIR, *Ph.D., Honorary Member of the Royal Agricultural Society of England**.

AS the principal object of this paper is to draw the attention of practical men to the conditions which effect a change in their dairy produce, it has been written in a more elementary form than would have been done, had its purpose merely been to communicate facts to scientific chemists. The object of the Chemical Society is to examine the applications of chemistry to practice, as well as to assist in the advance-

* Communicated by the Chemical Society; having been read January 17th, 1843.

ment of the abstract science. Many difficulties occur in the pursuits of a dairy farmer, which render his occupation precarious. Such difficulties arise entirely from an ignorance of the scientific relations of the practice in which he is engaged. I have endeavoured in the following paper to point out the causes which so often effect changes in the nature of his produce.

Boussingault and Lebel* instituted some experiments, with the view of proving that the composition of milk remains constant, when the food given to the cow contains the same amount of nitrogen: and their analyses established this fact, as far as regarded the casein in the milk; although its other ingredients varied very considerably, according to the nature of the food. The mean of eight analyses described in the first part of their memoir, gives the following composition for the milk of a cow:—

Casein	3·2
Butter	4·1
Sugar	5·1
Ashes	0·2
Water	87·4

The method employed by these chemists in their analyses did not differ from that used by Peligot in his examination of the milk of the ass†.

It consisted in treating the solid residue of a known weight of milk with æther, and afterwards with water. The loss in weight, after washing with æther, was estimated as butter, while that experienced by a similar treatment with water indicated the quantity of sugar; the dried insoluble residue being casein. The latter was incinerated, in order to obtain the ashes of the milk.

There is an error in this method of analysis, which seems to have escaped the attention of Boussingault and Lebel. It consists in the neglect of the inorganic matters dissolved by the water. In washing with water the mixture of casein and sugar, all the soluble salts of the milk are dissolved. The insoluble salts remain in the residual casein, and these alone were obtained by its incineration, whilst those dissolved by the water were neglected. Consequently, in the analyses of Boussingault, the quantity of sugar in the milk is always too high, and that of the inorganic ingredients too low.

The method of analysis employed in this paper was, with a few modifications, the same as the above. A weighed portion of milk, to which a few drops of acetic acid had previously been added, was evaporated to dryness at the heat of

* *Ann. de Chim. et de Phys.* lxxi. 65. † *Ibid.* lxii. 432.

boiling water (212°). The solid residue was digested with æther, thrown upon a weighed filter, and well washed with hot æther. The mixture of sugar and casein remaining on the filter being again dried at 212°, indicated by its loss in weight the quantity of butter dissolved by the æther. The æthereal solution itself was evaporated to dryness, and consisted of butter with colouring matter, more or less intense, according to the character of the food. The mixture of casein and sugar was washed with hot water, and the casein remaining on the filter, after being dried at 212°, was weighed, then incinerated, its ashes determined and deducted from the weight of the casein. The solution of sugar, being evaporated by a heat of 212°, yielded a residue consisting of sugar of milk and of the soluble salts of the milk. This residue, after being weighed, was incinerated and its ashes determined. These, deducted from the weight of the residue, yielded the amount of sugar, and added to the ashes of the casein, indicated the total amount of inorganic ingredients in the milk. The ashes of the filter were of course subtracted.

The cow which was made the subject of the following experiments is of the breed of short horns. I am not aware of the number of days since she was delivered of her last calf. When the experiments were instituted, she was in good milking condition. In order to estimate the average amount of her milk, I measured it for several days previous to the experiments. During this time she subsisted upon aftergrass; the meadow being about half a mile distant from the cow-house.

	Morning's milk.	Evening's milk.
October 5	5 quarts	$4\frac{1}{2}$ quarts
... 6	5 ...	4 ...
... 7	$4\frac{1}{2}$ ...	5 ...
... 8	5 ...	4 ...
... 9	$5\frac{1}{4}$ ...	4 ...

The weather was fine for the period of the year; but the nights being rather cold, on the evening of the 7th I directed that the cow should be driven to the house, and remain there during the night. In the morning it was put out to grass, but brought back in the evening. On the evening of the 9th I commenced the analyses, and followed them up in consecutive days. In every case the specimen of milk analysed was taken from the milk-pail, after the cow had been thoroughly milked, and the milk well stirred. This precaution was necessary, because the separation of the cream from the milk takes place in part in the udder of the cow. Hence the milk

last drawn from the udder contains much more cream than that first obtained*.

1st day. The cow, fed in the meadow upon after-grass during the day, was driven home to the cow-house in the evening. The milk then obtained amounted to four quarts. A portion of this was subjected to analysis.

Specific gravity of milk, 1034.

11·128 grammes of milk gave—		In 100 parts.
Casein	·611	5·4
Butter	·404	3·7
Sugar of milk . .	·429	3·8
Ashes	·068	0·6
Water	9·616	86·5
	11·128	100·0

During the day the cow had considerable exercise. Before being milked, it had half a mile to walk from the meadow. The nourishment in after-grass being small compared with fresh grass, the animal had to eat a greater quantity than it otherwise might have done, and consequently had to traverse more ground in order to procure it. The exercise which it thus received, by increasing the number of its respirations, must have occasioned a greater supply of oxygen to the system. This oxygen, as we shall afterwards show, unites with the butter and consumes it; consequently less butter is contained in the milk of the cow than would have been the case had its pasture been rich grass.

But, after being removed into the shed, less oxygen was respired, and the warmth of the house was equivalent to a certain amount of unazotised food†. The animal received nothing to eat during the night, and therefore the milk of the morning must have been derived from the after-grass consumed during the day. This milk measured four and a half quarts.

Specific gravity, 1032.

15·280 grammes yielded—		In 100 parts.
Casein	0·610	3·9
Butter	0·864	5·6
Sugar of milk . .	0·468	3·0
Ashes	0·091	0·5
Water	13·247	87·0
	15·280	100·0

* Schübler says that the milk last drawn contains three times as much cream as that first procured.

Dr. Anderson (Dickson's Practical Agriculture, vol. ii. p. 517) found the cream in the last cup of milk drawn from the udder, compared with that of the first cup, in the proportion of 16 to 1.

† In this paper we take for granted that the leading features of Liebig's 'Animal Physiology' are acknowledged as true.

The butter, as we might have expected, is in larger proportion than in the previous analysis. The amount of casein is smaller.

We shall defer the consideration of the causes which produce a variation in the quantities of the latter constituent.

2nd day. The object of this day's experiment was to discover whether an increase of butter would be procured by feeding the cow with after-grass in the stall. It refused, however, to eat this food, and being removed from its companions, struggled for several hours to regain its liberty. To render it tranquil, a companion was introduced into the same stall, and it was then induced to consume 28 lbs. of good hay and $2\frac{1}{2}$ lbs. of oatmeal. The milk of the evening measured $3\frac{1}{2}$ quarts.

Specific gravity, 1031.

22·684 grammes yielded—

		In 100 parts.
Casein . . .	1·124	4·9
Butter . . .	1·150	5·1
Sugar . . .	0·867	3·8
Ashes . . .	0·137	0·5
Water . . .	19·406	85·7
	22·684	100·0

The milk of the morning amounted to 4 quarts; but, owing to an accident, was not analysed.

3rd day.—A. The cow was kept in the shed, and consumed 28 lbs. of hay, $2\frac{1}{2}$ lbs. of oatmeal, and 8 lbs. of bean-flour. The milk of the evening amounted to 4 quarts = 10·34 lbs.

Specific gravity, 1034.

23·160 grammes gave—

		In 100 parts.
Casein . . .	1·262	5·4
Butter . . .	0·905	3·9
Sugar of milk	1·112	4·8
Ashes . . .	0·136	0·5
Water . . .	19·745	85·4
	23·160	100·0

B. The quantity of milk obtained in the morning amounted to $4\frac{1}{2}$ quarts = 11·61 lbs.

Specific gravity, 1032.

19·445 grammes of milk gave—

		In 100 parts.
Casein . . .	0·758	3·9
Butter . . .	0·888	4·6
Sugar . . .	0·877	4·5
Ashes . . .	0·129	0·7
Water . . .	16·793	86·3
	19·445	100·0

4th day.—A. The cow, kept in the stall as before, received this day 24 lbs. of potatoes (steamed), 14 lbs. of hay, and 8 lbs. of bean-flour. She gave in the evening 5 quarts of milk = 12·9 lbs.

Specific gravity, 1033.

17·820 grammes of milk gave—

		In 100 parts.
Casein . . .	0·707	3·9
Butter . . .	1·190	6·7
Sugar of milk	0·815	4·6
Ashes . . .	0·104	0·6
Water . . .	15·004	84·2
	17·820	100·0

B. The milk of the morning amounted to 4 quarts = 10·32 lbs.

Specific gravity, 1032.

19·641 grammes of milk yielded—

		In 100 parts.
Casein . . .	0·535	2·7
Butter . . .	0·978	4·9
Sugar of milk	0·991	5·0
Ashes . . .	0·116	0·5
Water . . .	17·021	86·9
	19·641	100·0

5th day.—A. The cow, kept as before, consumed 14 lbs. of hay and 30 lbs. of potatoes (steamed). She gave in the evening $5\frac{1}{8}$ quarts of milk = 13·18 lbs.

Specific gravity, 1030.

18·141 grammes of milk yielded—

		In 100 parts.
Casein . . .	0·716	3·9
Butter . . .	0·845	4·6
Sugar of milk	0·713	3·9
Ashes . . .	0·099	0·5
Water . . .	15·768	87·1
	18·141	100·0

B. The milk of the morning amounted to $4\frac{3}{4}$ quarts = 12·20 lbs.

Specific gravity, 1030.

16·740 grammes yielded—

		In 100 parts.
Casein . . .	0·600	3·5
Butter . . .	0·835	4·9
Sugar . . .	0·648	3·8
Ashes . . .	0·082	0·5
Water . . .	14·575	87·3
	16·740	100·0

Before proceeding to the consideration of these experi-

ments, it is necessary that we should examine the composition of the various kinds of food given to the cow. The cow received, during the course of the experiments, grass, oatmeal, hay, beans, and potatoes. The following analysis exhibits the composition of these various substances.

	Hay. Boussingault.	Oats. Boussingault.	Beans. Playfair.	Potatoes. Boussingault.
Carbon . .	38·47	41·57	38·24	12·30
Hydrogen .	4·20	5·25	5·84	1·74
Oxygen . .	32·51	30·10	33·10	12·04
Nitrogen .	1·26	1·80	5·00	0·32
Ashes . .	7·56	3·28	3·71	1·40
Water . .	16·00	18·00	14·11	72·20

In a specimen of beans analysed by Boussingault, only 4 per cent. of nitrogen was found. But in the bean-flour, which I used in the experiments, there was as much as 5 per cent. It is obvious that if we multiply the quantity of nitrogen by 6½, the product will be the amount of casein or albumen in the various kinds of food; and further, by deducting this, together with the water and ashes, the remainder must indicate the quantity of unazotised matter.

	Albumen or casein.	Unazotised matter.
Hay . .	7·81	68·63
Oats . .	11·16	67·56
Beans . .	31·00	51·18
Potatoes .	1·98	24·42

Finally, according to Liebig, good hay contains 1·56 per cent. of a fatty or waxy substance. Braconnot found 0·70 per cent. of a similar substance in beans. Vogel found 2 per cent. in oats, and Liebig 0·3 per cent. in potatoes.

Dumas, in an announcement to the French Academy, has lately advanced the theory, that the fat of animals is wholly derived from the fatty matter contained in their food. This opinion has been very ably combated by Liebig, who refers to an analysis of milk executed by Boussingault, and shows that much more butter was contained in it than could be accounted for by the fat in the food taken. As the theory of the formation of fat is of the first importance in the practice of dairy farming, we will shortly examine Dumas's theory with reference to the preceding experiments.

1. On the 2nd day the cow received 28 lbs. of hay, which contains 0·436 lb. of fat and 2½ lbs. of oatmeal, containing 0·050 lb. of the same constituent. The cow produced (calculating according to its specific gravity) about 19 lbs. of milk, in which was 0·969 lb. of butter. But the food altogether

contained only 0·486 lb. of fat, so that 0·483 lb. of butter must have been produced from other sources.

2. The food received by the cow on the 3rd day consisted of 28 lbs. of hay, $2\frac{1}{2}$ lbs. of oatmeal, and 8 lbs. of bean-flour.

28 lbs. of hay contain . . .	0·436 lb. of fat.
$2\frac{1}{2}$ lbs. of oatmeal contain .	0·050 lb. of fat.
8 lbs. of beans contain . .	0·056 lb. of fat.
In the food	0·542 lb. of fat.

The milk of the evening amounted to 10·34 lbs., and contained 0·4 lb. of butter; that of the morning to 11·61 lbs., and contained 0·5 lb. of butter. The butter in the milk amounted, therefore, to 0·9 lb., of which only 0·542 lb. could possibly have been furnished by the food, assuming that the fat in the food could be converted into butter.

3. The cow received on the 4th day 14 lbs. of hay, 8 lbs. of beans, and 24 lbs. of potatoes.

14 lbs. of hay contain . . .	0·218 lb. of fat.
8 lbs. of beans contain . .	0·056 lb. of fat.
24 lbs. of potatoes contain .	0·072 lb. of fat.
	0·346

The evening's milk amounted to 12·9 lbs., and contained 0·86 lb. of butter; that of the morning to 10·32 lbs., and contained 0·50 lb. The cow, therefore, furnished during the day 1·36 lb. of butter. The fat in the food amounted only to 0·346 lb., and therefore 1·064 lb. must have been received from other sources.

4. On the 5th day the cow received 14 lbs. of hay and 30 lbs. of potatoes.

14 lbs. of hay contain . . .	0·218 lb. of fat.
30 lbs. of potatoes contain .	0·090 lb. of fat.
	0·308

The milk of the evening amounted to 13·18 lbs., and contained 0·606 lb. of butter; that of the morning to 12·20 lbs., containing 0·597 lb. of butter. The cow, therefore, furnished 1·203 lb. of butter. The fat in the food amounted only to 0·308 lb. Hence 0·895 lb. of butter must have been produced from other sources.

From these calculations it must be obvious, that the butter in the milk could not have arisen solely from the fat contained in the food. Hence it must have been produced by a separation of oxygen from the elements of the unazotised ingredients of the food of the animal, in the manner pointed out by Liebig.

We remark striking variations in the quantity of butter in the preceding analysis, and a similar result occurred in the

experiments of Boussingault. In the milk of the first day there is a small amount of butter. The cow had been exposed in the field during the day, and hence required a greater quantity of unazotised food to support the heat of its body than would have been necessary had it been protected from the cold. But in the evening it was removed into a warm and well-littered stall, where the warmth thus communicated was equivalent to a certain amount of food; and hence we find, that the milk of the morning was considerably richer in butter. It is uniformly found to be the case, that a stall-fed cow yields more butter in its milk than one fed in the field. Besides the warmth of the shed, less butter is consumed by the oxygen of the air. In the stall the respirations of an animal are much less frequent than in the field, and consequently less oxygen enters into its system. The great care of all dairy farmers is to prevent an excess of this gas from entering the body. Hence the practice of milking in the field those cows which are distant from home, and of driving home to be milked only such cows as are close to the shed. The exercise required in walking home causes an increased play of the respiratory system, and therefore increases the amount of oxygen inspired. This oxygen unites with part of the butter and consumes it. The greatest care is taken by all good dairymen to allow the cows to walk home at their own pace, and never to accelerate it. By this means only a small amount of oxygen enters the system. When a cow is harassed and runs to escape from the annoyance, its milk becomes very much heated, diminishes in volume and in richness, and speedily becomes sour. This is a fact well known to all dairymen. During running the cow respires a large quantity of oxygen. This unites with the butter, and the heat evolved by its combustion elevates the temperature of the milk and evaporates part of its water. The acetous fermentation is also induced, and cannot be restrained. For this reason cows are not turned into the fields in very hot weather, when the flies are apt to annoy them and produce restiveness. During such weather, it is not an uncommon practice to feed the cows in the stall during the day and turn them out to grass at night. By this means they are kept tranquil, and prevented from respiring a large amount of oxygen. There cannot be any doubt that the practice of stall-feeding cows during winter or in cold weather must conduce very much to the formation of butter; but in summer, when the pastures are rich and near the dairy, the slight exercise which they receive increases their health, and with it their appetite. They are thus induced to eat more

food than they would do in the stall, and they consequently receive a greater flow of milk. The loss experienced by the greater absorption of oxygen is more than compensated for by the increased appetite of the animal.

In these experiments it will be remarked, that potatoes were favourable both to the flow of milk and formation of butter. This quite accords with practical experience. They abound in starch, and therefore furnish the substance from which butter is formed. The increase of butter in the milk of the fourth day is very striking; on this day 24 lbs. of potatoes formed part of the food. The butter is also in large quantity in the milk of the fifth day, though not so much so as in that of the fourth, though 6 lbs. of potatoes in excess were consumed. In these 6 lbs. of potatoes only $1\frac{1}{2}$ lb. of dry unazotised matter were furnished, which could not compensate for 8 lbs. of beans (containing 4 lbs. of dry unazotised matter) which had formed part of the diet of the preceding day. The result is, therefore, exactly as might have been anticipated. When the food contained much starch, the sugar of milk increased in quantity as well as the butter. The large amount of butter in the milk of the second day is singular, and makes us regret the accident which happened to the milk of the morning and prevented its analysis. A remark has often been made to me by practical men (how far it is true I know not), that the milk of the morning is generally richer than that of the evening. As far as the limited number of analyses here given warrants any conclusion, there would seem to be some accuracy in this observation. The cause that it should be so is apparent: during the day, when exercise is taken, the number of respirations is frequent, and a large amount of oxygen enters the system—this is unfavourable to the formation of butter; but at night, during sleep, the respirations are slow, and the amount of oxygen respired is trifling. Such a condition must favour the separation of oxygen from starch to supply the deficiency.

All practical experience is against the theory of Dumas, regarding the formation of butter in milk, and of fat in cattle. In Scotland the system of stall-feeding cows is carried to a great extent. The Glasgow dairymen feed their cows in warm stalls, giving them malt refuse, a few pounds of beans, steamed turneps and potatoes, and as much pot ale (residuum after distillation, commonly called wash) as they will drink. The malt refuse consists of starch, gum, and a little saccharine matter, but it is not known to contain fat. This refuse is the principal food, and is very favourable to the production of butter, evidently from its great excess of unazotised matter. The beans

furnish the nitrogenous matter, in which the other foods are deficient; and as they contain casein ready formed, they assist in the formation of the milk. Their value is fully appreciated by all Scotch dairymen, although their use is little known in this country. The pot ale contains sugar and alcohol, and therefore contributes to sustain the heat of the body. It thus enables the other food to be formed into butter. But its principal use seems to be in diluting the secretions. Pure water does not readily enter the blood; we know that it destroys the blood-globules. But acid water does not do so, and this pot ale is generally very acid. Hence it dilutes the secretions. The great object, therefore, in this kind of feeding, is to give the cows as much unazotised food as possible; and the food which is preferred, consists of those very kinds in which fat exists in the least proportion. Porter and beer are well known to be favourable to the production of butter in the milk both of women and of cows; yet these fluids do not contain fat. We are, therefore, justified in asserting that practice is opposed to the theory of Dumas, but highly favourable to that propounded by Liebig.

During stall-feeding we have it in our power to alter the composition of milk, even with respect to the casein. Thus in the second day the cow received in its food $2\frac{1}{2}$ lbs. of albumen, or of a substance of the same composition (28 lbs. of hay, $2\frac{1}{2}$ lbs. of oatmeal). It produced 19 lbs. of milk, in which was 0·93 lb. of cheese. The next day it received 5 lbs., or double the quantity, of albumen in its food, and the milk, which amounted to 22 lbs., contained 1 lb. of casein*. Still theory would have led us to anticipate a larger increase than actually took place. The circumstance which favoured the production of casein in the milk of the first day will afterwards be considered.

The cow received in the food given on the fourth day, 4 lbs. of casein and albumen (14 lbs. hay, 8 lbs. beans, 24 lbs. potatoes), and yielded 23·22 lbs. of milk, which contained 0·75 lb. of casein. The fifth day she received considerably less albumen in her food, viz. 1·7 lb. (14 lbs. hay and 30 lbs. potatoes), but the amount of casein in the milk, though in less per centage than before, was equal in amount to the other, from there being a greater flow of milk. The milk amounted to 25 lbs., of which about 0·94 lb. was casein. Here, although the food did not contain much casein, it was such as to induce a great flow of milk, and the casein must have been derived from the albumen of the blood. Had the cow been continued on this food, there cannot be a doubt that the

* Calculated according to the composition of the evening's milk, as that of the morning had not been analysed.

quantity of casein in the milk must either have diminished, or the cow must have lost condition in giving this substance at the expense of its tissues.

The value of these experiments is certainly very much diminished by not being extended over a series of days on each kind of food. But in England, where the price of æther is so exorbitantly high, the expense of such experiments is a serious consideration for a private individual. As they were conducted under the same conditions with respect to temperature and exercise, an indication of the effects produced by the various foods must have been obtained, although the final effects have escaped detection.

Neglecting the second day, in which an abnormal increase of casein was produced by an accidental circumstance, we find the milk of the third day contained 5·4 per cent. of casein, that of the fourth 3·9 per cent., and of the fifth also 3·9 per cent. The food consumed on the third day contained a very large amount of casein, and this was immediately followed by an increased amount in the milk. Some peculiar cause favoured the flow of milk on the fifth day, in spite of the small quantity of albumen in the food; the milk derived its casein from other sources. The milk of the second day contained 4·9 per cent. of casein, while that of the fourth day possessed only 3·9 per cent. of the same constituent, although on that day the cow had received 4 lbs. of albumen in its food, and on the former day only one half or 2 lbs.

Such are the changes which constantly occur to the dairy farmer, and cause variations in the value of his milk, even when the conditions of feeding seem to be the same. It is for us to determine to what these seemingly discordant results are due.

In experiments such as these, we must remember that the animal body is not a mere chemical laboratory, in which a chemist may operate as he pleases. But there is a power,—vitality,—superior to his, and it is only by its concurrence that the changes which he desires are effected.

Now, on the second day the animal struggled violently to regain its liberty, and consequently expended much matter in the production of force. It is difficult to conceive that any waste of tissues can take place, without an alteration in their chemical composition. Still we cannot deny (in the present state of our knowledge), that an alteration in form might effect a waste, as well as a change in composition. We know little or nothing of the nature of secretion. All we know is, that certain glands have the power of appropriating particular parts of the organism or of food, and of producing fluids, which

either perform some new functions in the system, or are separated from it; but we are entirely ignorant how these secretions are produced. Scherer has indeed pointed out that albumen may be converted into casein by digestion with caustic potash; and it is possible that this may be the process employed to form it in the organism. On this view, we might suppose that the waste of the tissues operated indirectly in its production. By their waste their alkaline constituents are liberated, and might act upon the albumen of the blood by converting it into casein. Be this as it may, there are many facts which induce us to believe that the waste of the tissues does favour the production of casein in the milk. The milk of a cow, fed in the stall, is not only absolutely but relatively poorer in casein than one fed in the field, where exercise increases the transformation of its tissues. During parturition all the muscles are thrown into a violent state of action. As the labour in a cow continues for many hours, there must be a great waste of tissues in the production of the force necessary to occasion these muscular exertions. Such being the case, if our view be correct there ought to be in the milk of a cow, immediately after parturition, an abundance of casein: and every one knows that such milk is quite thick with cheese. We are indebted to Boussingault for an analysis of the milk of a cow before the calf had been allowed to suck. He found it to contain as much as 15 per cent. of casein, while the milk of the same cow analysed a few days after its calving contained only 3 per cent. of the same substance; therefore only one-fifth the quantity*. If then the waste of the tissues tends, directly or indirectly, to increase the amount of casein in the milk, then we are at no loss to understand why the milk of the second day should be unusually rich in casein.

Beans contain 31 per cent. of casein ready formed. Hence it is that they have been found so practically useful in aiding the formation of the milk.

The conditions necessary for the production of casein in the milk, are different from those which are favourable to the formation of butter. When butter is the principal object desired, the cow cannot be put upon too rich pastures. But in all cheese districts, it is agreed that *poor* land is best adapted for cheese. Land is called poor, not when the grass is deficient in nitrogenous bodies, but in constituents desti-

* It might be objected to the view given, that the analysis of this milk indicates a very small amount of inorganic ingredients. Boussingault found only 0·3 per cent. To this it may be answered, that the alkalies which favoured the formation of casein are soluble, and therefore were neglected in Boussingault's analysis, by being included along with the sugar of milk. (*Ann. de Ch. et de Phys.* lxxi. p. 72.)

tute of nitrogen. The "equivalent," as farmers term it, is higher; that is, the cattle are compelled to eat a greater quantity of poor than of rich grass, in order to sustain animal heat. They have also to traverse more ground to procure their food; this causes an increased respiration of oxygen and waste of the tissues. Hence the appetite of the animal is increased, and a larger quantity of food is consumed. If the view already given be correct, the waste of the tissues aids in the supply of casein to the milk. One great object in cheese-farms is to induce the cows to eat a large quantity of food, and for this purpose, in large farms, they are tempted with new pastures every day. The Cheddar, Cheshire and Stilton cheeses contain a considerable quantity of butter. In a celebrated cheese-farm, a few miles from Bridgewater, where Cheddar cheese is made in great perfection, I found it to be the practice (a prevalent one, I believe) to drive the cows in the morning to the pastures on dry sandy soil, and in the evening to those situated on soft peaty soil. The grass of the sandy soil being poor, the cows traversed considerable ground in procuring it. They therefore eat a quantity more than they would have done on rich land, where there is little exertion required in taking the food. By this means a large quantity of cheese was procured in the milk. But during the night they were allowed to feed on rich pastures, fitted for the formation of butter, and as the darkness prevented them from wandering, little oxygen was respired to consume the butter formed, or to prevent its formation. The milk of the evening and morning being always mixed together in the preparation of the cheese, a proper proportion of both constituents was thus procured. In districts where inferior cheeses are manufactured, that is, in which the farmer depends upon his butter as much as upon his cheese, he is ignorant of the value of poor land. Thus it is that there is occasionally a contradiction amongst dairymen with respect to this point, though there is none with those who depend wholly on their cheese, and care little for the butter, except as a means of enriching the former.

Chevallier's analysis of woman's milk (the only one of which I am aware) indicates a very small quantity of butter. As attention to diet and exercise must be very important to nurses, when we consider the trivial causes which produce a variation in the composition of milk, I was desirous of ascertaining whether the quantity of butter could be made greater than in the analysis given by Chevallier. Accordingly I selected a farmer's wife, a strong healthy female of twenty-eight years of age, who had been delivered of her third child. On the 19th day after her confinement she remained in bed, and thus di-

minished the amount of oxygen respired, and was then fed upon gruel (oatmeal and water). On the 21st sufficient milk for analysis was procured.

23·945 grammes yielded—

Casein	0·3695	1·54
Butter	1·0315	4·30
Sugar of milk. .	1·3770	5·75
Ashes	0·1270	0·53
Water	21·0400	87·88
	23·9450	100·00

The butter in this milk was therefore quite equal to that in the milk of a cow. As the diet thus influences the composition of the milk, may not the frequent occurrence of ricketty children in the higher classes of life be due to the circumstance of the mothers living principally upon white bread—bread, therefore, from which phosphates have been in a great measure removed? Had the woman been at her usual exercise, it must of course have diminished. The milk of a woman possesses a very sweet taste, and is remarkable for its great amount of sugar of milk. In this and in other respects it closely resembles that of the ass. The following analyses exhibit the composition of the milk of various animals :—

	Woman.		Ass.		Cow.		
	Henry and Chevallier*.	Playfair†.	Peligot‡.	Henry and Chevallier.*	Boussingault‖.	Henry and Chevallier*.	Playfair§.
Casein . . .	1·52	1·54	1·95	1·82	3·2	4·48	4·0
Butter . . .	3·55	4·30	1·29	0·11	4·1	3·13	4·6
Sugar of milk	6·50	5·75	6·29	6·08	5·1	4·77	3·8
Ashes . . .	0·45	0·53	..	0·34	0·2	0·60	0·6
Water . . .	87·98	87·88	90·47	91·65	87·4	87·02	87·0

Before concluding this paper, I take the opportunity of making a few remarks to *practical men* on the mode of preserving milk, as this is a subject on which questions have been often sent to me.

Milk consists of casein, of sugar of milk, and of certain salts dissolved in water, in which are suspended little globules of fat or butter. These globules are surrounded by a shell or skin, which is supposed (by Otto) to be coagulated casein. The soluble casein, being a nitrogenous body, is very apt to run into putrefaction. In summer it does not do so readily, because the temperature being elevated, the sugar of milk is converted apparently into grape sugar by the agency of lactic acid, then

* *Journal de Pharmacie*, xxv. 333 et 401. † *Suprà.*

‡ *Ann. de Ch. et de Phys.* lxii. 432.

‖ *Ann. de Ch. et de Phys.* lxxi. 65.

§ Average of analyses of the milk of a cow in the field.

into alcohol, and the alcohol into acetic acid. These changes are induced by a primary action of oxygen upon the casein. This action is then imparted to the other constituents, the atoms of which being once set in motion, readily undergo the changes described. The acetic acid being formed by the agency of air on the alcohol, acts upon the soluble casein and coagulates it, or renders it insoluble. It is thus removed from the action of the oxygen of the air, and may be kept for some time without entering into putrefaction. Such are the changes which milk undergoes in summer, but they are quite different in winter.

In winter the first action is that of oxygen upon the casein. The temperature is not sufficiently elevated to cause vinous fermentation. The decay of the casein generally passes over to putrefaction, that is, the atoms are transformed more rapidly than they unite with oxygen. A putrid smell now arises.

Good butter cannot be made from milk which has undergone this change. The cause is, that butter always contains a certain quantity of casein which it is difficult to remove. When incipient putrefaction has taken place, it cannot be arrested by ordinary means, and imparts itself to the bodies with which it is in contact. It is for this reason that the greatest part of the butter manufactured in winter has a rank putrid taste.

The principal object in view in the preservation of milk in winter, is to prevent the commencement of this putrefaction. One method has been termed *scalding* the milk, and is generally used in dairies. It consists in heating the milk until the oxygen of the air acts upon the casein, and forms a pellicle on its surface. The milk should then be left to perfect repose. The pellicle excludes the air from the soluble casein. The partial oxidation by which the pellicle was produced, is effected at too high a temperature to enable the decay to pass into putrefaction. When this operation is skilfully performed, the milk remains quite good for four or five days. But there is a risk of failure in this process, and it is only adapted for small dairies.

The best method, which I have seen used in practice with much success, seems to be to induce the acetous fermentation in the milk. For this purpose, the cream or milk, being placed in a proper vessel, should be surrounded with hot water. The heat which I find to answer best is from 100° to 110°. A cloth may be thrown over the whole to retain the heat, and as the water cools, it should be removed and replenished with hot water of the above temperature. In a few hours the cream acquires the smell and taste of vinegar. The changes which I have described above ensue. In large dairies

a portion of this soured cream or milk may be added to fresh cream or milk, which should be kept in a room possessing a temperature of 60°. By adding this soured cream to the fresh milk, we furnish an acid, by which the sugar of milk is converted into grape sugar. The curd then acts upon the grape sugar, and converts it into alcohol. The latter by oxidation becomes acetic acid, and thus the whole mass of milk is rendered sour, the casein coagulated, and therefore protected from immediate putrefaction. The butter made from such soured milk is quite sweet and destitute of that rank taste which distinguishes our winter from summer butter. But if incipient putrefaction has once begun in the milk, all this will be of no avail, because it is communicated to the insoluble casein. Milk perfectly fresh must therefore be used. Fresh milk soured in this way will last for many days, and give risings of cream for a considerable time. This practice, as far as I am aware, is not a general one, though it is well worthy of adoption. In summer of course no such operation is requisite, as it is done at a sacrifice of the skimmed milk. One great cause of the putrefaction in milk is the want of absolute cleanliness in the dairy. If a drop of milk fall on the table, it should be dried and washed off with care, for its putrefaction causes the evolution of a putrid gas, and this *imparts* its state of putrefaction to the remainder of the milk.

With respect to making butter, scientific explanations can be of little use to practical men. The theory of churning is very simple. By agitation, the globules of butter are broken, and made to unite together into a mass. The introduction of air during churning, aided by the heat at which the cream or milk is, occasions the formation of lactic or acetic acid, and this coagulates the casein, and thus assists the separation of the butter. In summer, when the heat prevents the ready coherence of the butter, a quantity of cold spring water thrown in, after the butter-milk has formed, often effects the desired end. The temperature is thus depressed, the butter rendered solid and more coherent, while the air contained in the water aids in the formation of acid and coagulation of the casein. The only thing, in a scientific point of view, to attend to after the separation of the butter, is to free it from butter-milk or casein. If the casein be suffered to remain, putrefaction ensues, and the butter acquires a rank putrid taste. Its separation is therefore of the first moment.

The cause of the superiority of certain foreign butter, which retains its flavour and taste for a considerable time, is more due to its freedom from casein than to any mystery in its mode of preparation.

XXXV. *Places of Saturn computed by* Hansen's *Formula. By* S. M. Drach, *Esq., F.R.A.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The new analytical methods proposed by M. Hansen having of late attracted much attention, I have for some time employed myself in computing several places of Saturn rigorously from his formula; selecting for the dates those specified in the Greenwich Observations for 1837–40, as the mean day of the monthly observations. M. Hansen having assumed the same elements as M. Bouvard (whose tables of Saturn are now used for the Nautical Almanac), we derive the great advantage of exactly testing the value of both methods by reference to observation. I have therefore made the computations for the London mean noons = Paris ones + $9^{m}\ 21^{s}\cdot5$ for the 21 dates of the annexed table, in the following manner:—

The mean anomalies of Jupiter and Saturn gave the arguments for all M. Hansen's perturbations in longitude, to which adding M. Bouvard's Georgian part of the great equation, viz.

$$-\,34''\cdot58\ (\text{sex.})\ \sin\{3n''t + 3\epsilon'' - nt - \epsilon - 95^{gr}\cdot08\}$$

and applying the sum to g' = Saturn's mean anomaly, there resulted the corrected anomaly, wherewith the equation of the centre and elliptical value of the radius vector was computed. M. Bouvard's tables furnished the perturbations produced by ♅, and adding the constant perih. long. 89° 8′ 20″, the sum gave the true perturbed longitude in the orbit from the mean equinox of 1800 = v. After allowing for the reduction to the ecliptic, M. Bouvard's value of the precession = $0^{gr}\cdot015463$ = $50''\cdot1001$ (sex.) and the ephemeridal equation of the equinoxes (p. 266) were applied, whence the true longitude from the true equinox was ascertained, and its comparison with the Nautical Almanac value exhibited in column 3. The other two coordinates are exhibited in columns 6 and 9 in the same manner.

The errors in Hel. lat. are immediately extracted from the Greenwich Observations, and likewise those in Hel. long. for 1837–39. The volume for 1840 being deficient in these, they were approximatively supplied as follows. The planet never being as far as $1^{h}\ 4^{m}$ in Geo. R.A. from the solstitial colure, the variation in declination has little influence on the R.A.; hence the effect of the small monthly average error in the latter on the Hel. long. could be found nearly by simple proportion: thus in No. 16, March 9 − March 8 = + $9^{s}\cdot40$ in Geo. R.A. and + $108''\cdot6$ in Hel. long.

∴ mean error in former $= -0^{s}{\cdot}823$ produces $-\frac{823}{9400}108''{\cdot}6$
$= -9''{\cdot}51$ error in Hel. long.

The accompanying table, which has been verified in several particulars, shows that A − H in longitude is nearly

TABLE.

No.	Date.	Hel. Long.			Hel. Lat.			Log. R. V.
		A − H.	A − G.	H − G.	A − H.	A − G.	H − G.	A − H.
	1837.	″	″	″	″	″	″	0·0000
1	Feb. 12.	+21·61	+2·52	−19·09	+13·50	−15·11	−28·61	−240
2	Apr. 13.	+20·97	+0·34	−20·65	+12·99	−15·76	−28·75	−248
3	May 15.	+21·92	+1·31	−20·61	+12·47	−15·48	−27·95	−251
4	June 14.	+20·16	+0·54	−19·62	+12·06	−15·19	−27·25	−243
	Mean.	+21·16	+1·18	−19·98	+12·75	−15·39	−28·14	−245
	1838.							
5	Feb. 12.	+19·14	−3·88	−23·02	+ 9·66	−16·12	−25·78	−249
6	Mar. 16.	+18·67	−2·93	−21·60	+ 9·45	−13·72	−23·17	−193
7	Apr. 16.	+19·00	−3·50	−22·50	+ 9·25	−15·08	−24·33	−250
8	May 17.	+19·34	−2·95	−22·29	+ 9·02	−15·20	−24·22	−239
9	June 12.	+18·92	−3·91	−22·83	+ 8·69	−14·82	−23·51	−238
10	July 8.	+18·72	−3·93	−22·45	+ 8·29	−14·30	−22·59	−221
	Mean.	+18·97	−3·53	−22·50	+ 9·06	−14·87	−23·93	−233
	1839.							
11	Feb. 25.	+20·16	−5·66	−25·83	+ 3·99	−13·99	−17·98	−146
12	Apr. 19.	+18·14	−8·11	−26·25	+ 4·55	−15·23	−19·78	−124
13	May 18.	+18·15	−6·81	−24·96	+ 4·13	−15·27	−19·40	−109
14	June 18.	+18·36	−4·68	−23·04	+ 3·73	−14·60	−18·33	−091
15	July 13.	+18·69	−4·75	−23·44	+ 3·38	−14·67	−18·05	−086
	Mean.	+18·70	−6·00	−24·70	+ 3·96	−14·75	−18·71	−111
	1840.							
16	Mar. 9.	+19·05	−9·51	−28·56	+ 0·07	−13·49	−13·56	+051
17	Apr. 16.	+19·08	+9·24	− 9·84	− 0·49	−13·85	−13·36	+102
18	May 29.	+20·40	+9·27	−11·13	− 1·18	−15·70	−14·52	+133
19	June 16.	+18·16	+3·65	−14·51	− 1·35	−15·25	−13·90	+150
20	July 17.	+20·02	+4·49	−15·53	− 1·79	−14·90	−13·11	+168
21	Aug. 4.	+19·04	+8·87	−10·17	− 2·07	−14·40	−12·33	+196
		+19·29	+4·34	−14·95	− 1·14	−14·60	−13·46	+133

constant, the general mean being $+19''{\cdot}42$, a difference which an addition of 1° to the epoch of $-2845''{\cdot}8 \ldots \sin(5g' - 2g + 246°\ldots)$ would nearly remove*. The differences in the latitude and log.

* If the precession be taken as in Poisson's *Méc.* ii. p. 195 = $50''{\cdot}2343$ as on the moveable ecliptic, this would diminish the error A − H in long.

rad. vect. assume a periodic form; and as a change of 1° in the long. occasions a change in the lat. ranging from $-55''$ to $35''$, another cause must be found for the discrepancy. Now the only considerable part affecting this subject is the term in t, which ranges from $-15''\cdot213$ in No. 1, to $-31''\cdot000$ in No. 21. These extremes agree with the

$$\left\{\begin{matrix}\text{N. A.}\\ \text{G. O.}\end{matrix}\right\}\text{if } + 0''\cdot8184\, t_1 \sin\left\{v_1 + \begin{matrix}322^\circ.\,23'\\ 312^\circ.\,58'\end{matrix}\right\}\text{and}$$

$$+0''\cdot8184\, t_{21} \sin\left\{v_{21} + \begin{matrix}364^\circ.\,14'\\ 306^\circ.\,19'\end{matrix}\right\}$$

be assumed for those equations, *i.e.* if with the latter 309° and not 349° be the true epoch of M. Hansen's equation.

S. M. Drach.

London, 22nd June, 1843.

XXXVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 71.]

May 4, 1842. — A letter addressed to the President by Mr. Ick, F.G.S., on some superficial deposits near Birmingham, was first read.

While excavating that part of the New Junction Canal which passes through the valley of the Rea, at Saltley, a mile and a half north-east of Birmingham, the workmen at the depth of five feet came to a deposit of carbonaceous matter, consisting of compact peat, in which were imbedded rounded pebbles of white quartz, and branches as well as prostrated trunks of oaks, hazels and willows, the former being occasionally upwards of six feet in length. The wood exhibited various stages of carbonization, some specimens being reduced to a soft state, while others, "consisting of oak, were scarcely so much changed as the timbers of the Royal George." The author did not observe an instance of coniferous structure. About 150 yards from the river the deposit is two feet and a half thick, and contains abundance of hazel-nuts. The horn of a stag, probably of the *Cervus elephas*, which was found there, measured from the base to the broken tip of the extreme antler one foot seven inches, and eight and a half inches around the base, and the brow antler was nine inches in length. At the distance of twenty yards, where the peat was mingled with gravel, the core of the horn of an ox was found, one foot in circumference at the base, and one foot eight inches long.

At the bottom of the peat is generally a thin layer composed principally of angular particles of white quartz, beneath which occurs

by $\frac{1}{4}$ (from $4''\cdot981$ to $5''\cdot447$), and if we assume it $= 50\cdot3648$, as on the fixed ecliptic of 1800, this diminution would rise to $\frac{1}{4}$ (from $9''\cdot824$ to $10''\cdot774$).

the usual marine drift of the district, the greater part of the boulders contained in it, consisting of Lickey quartz-rock; and the whole rests on the new red sandstone. Above the carbonaceous bed is a stratum, from six to eighteen inches thick, of fine clay, frequently almost white, but in some instances of various shades of yellow and red. Upon the clay is occasionally a bed of coarse gravel composed of the usual Lickey pebbles, and over it occurs a pale red sand, which gradually passes upwards into a sandy vegetable soil. The average thickness of these overlying deposits is five feet.

At a spot about 250 yards from the river, the place of the peat is occupied by a bed of gravel composed principally of boulders from eight to ten inches in circumference; and above it lies the prevalent light-coloured clay eighteen inches thick. The next stratum, in ascending order, consists of very fine comminuted peat, with small fragments of hazel and oak twigs, the whole bearing the appearance of a drifted mass. The highest bed, immediately under the vegetable soil, is composed of sandy clay, and is about seven inches thick.

In some spots the lower vegetable deposit rests on a deeply orange-coloured, ferruginous clay, and where this has been removed, the action of water on the drift, Mr. Ick says, is very apparent, the larger pebbles standing in high relief exactly in the same manner as in the bottom of the present river, where a rapid current flows over the gravel.

Mr. Ick has traced this peaty deposit in places along the banks of the river towards Birmingham, through Deritend, particularly at Vaughton's Hole, where it is eighteen inches thick. It has also been penetrated in making wells and culverts in the lower part of Digbeth, and nuts and bones have been found there.

The next communication read is entitled "A Postscript to the Memoir on the occurrence of the Bristol Bone-Bed in the neighbourhood of Tewkesbury," by Hugh Edwin Strickland, Esq., F.G.S.

Since the reading of the former communication (Phil. Mag. S. 3. vol. xxi. p. 540.) Mr. Strickland has ascertained that the bone-bed occurs at least ten miles further north, or at Defford Common, in Worcestershire, making a total range of 104 miles. At this locality are some old salt-works belonging to the Earl of Coventry, and the shaft, which was sunk about seventy years ago to the depth of 175 feet, was emptied a few months since of the brine with which it is wont to overflow. At the bottom of the shaft, which descends through the lias into the grey marl of the triassic series, but without reaching the red marl, is a tunnel that follows the dip of the strata for about 160 yards. The shaft, Mr. Strickland says, consequently intersects the horizon of the "bone-bed," and among the rubbish thrown out, he found considerable quantities of the peculiar white sandstone with bivalves (*Posidonomya*), shown in his former paper to represent in Worcestershire the bone-bed of Aust and Axmouth; but he also found specimens of the sandstone charged with the same description of teeth, scales and coprolites so abundant at Coomb Hill and the localities just mentioned.

The occurrence of an abundance of pure salt water* within the area of lias, Mr. Strickland says, is an interesting phenomenon, and for a solution of it, he refers to Mr. Murchison's Account of the Geology of Cheltenham, p. 30.

A paper on the high Temperature of Wells in the neighbourhood of Delhi, by the Rev. Robert Everest, F.G.S., was then read.

The country around Delhi is remarkable for its great dryness; and if a line were to be drawn due west from the Jumna to the Indus, a distance of 400 miles, it would intersect no river, brook or spring, water being obtained only from wells. At Delhi these wells are generally about 35 feet deep; 40 or 50 miles to the westward the depth is from eighty to ninety feet; and beyond that distance to Hauri, 95 miles, it increases to 150 feet. Mr. Everest did not visit the country further to the west, but he believes that the wells have a depth of 150 feet or more.

The soil consists of a granitic alluvium, but the surface is covered in many places with saline efflorescences similar to those which the floods of the Jumna now leave behind them.

The results of the author's observations are given in the following tables:—

1. Well at Delhi, 42 feet to the bottom. This table shows the amount of annual variation.

	Temp. of water. °	Temp. of ext. air. °		Temp. of water. °	Temp. of ext. air. °
1833. Nov. 12.......	79	 76	1834. May 12.......	78·9	 78
Dec. 17.......	76	 62	June 17.......	80	...... 86·5
1834. Jan. 25.......	74·7	 68	July 25.......	80·9	...... 82·2
Mar. 2.......	76·8	 84	Sept. 2.......	81·3	 92
Mar. 29.......	77	 67	Sept. 29.......	81·5	 80

Average temperature: water, 78·61; external air, 77·57.

2. Wells to the W.S.W. of Delhi, ten to fifteen miles apart, the first being the furthest, or 90 miles.

1834.	Locality.	Depth to water. Feet.	Depth of water. Feet.	Total depth. Feet.	Temp. of water. °	Temp. of ext. air. °
Jan. 16.	Toolshaum...........	90	 7	 97	 82	 67
„ 17.	Baphoora	52	 28	 80	 82	 72
„ 19.	Dadnee	45	 15	 60	 81	 70
„ 20.	Billoti.................	52	 15	 67	 81	 68
„ 22.	Djuggur..............	44	 5	 49	 77	...... 71·5
				Average......70·6	80·6	

General mean of twelve wells: depth, 96 feet; temperature, 80°·2.

3. Wells situated to the west of Delhi.

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* An imperial gallon of the brine is stated to yield the following saline contents:—Chloride of sodium .... 5807·6 grains.

Sulphate of lime .... 195·8 grains.
Magnesia .... A trace.

Dr Hastings's Paper in the Analyst, vol. ii. p. 384.

1834.	Locality.	Dist. from Delhi. Miles.	Depth to water. Feet.	Depth of water. Feet.	Total. Feet.	Temp. Fahr. °	Temp. of air. °
Mar. 31. April 14.	Bahadurghur	20	30	30	60	75·2	76·2
„ 1.	Samplah	35	69	19	88	81·5	90·5
„ 3.	Moheim	65	96	39	135	82·8	95
„ 4.	Moordahal	75	96	44	140	75	89·5
„ 5.	Hausi	95	69	50	119	83·1	89
„ 7.	Hausi		115	45	160	79·8	67
„ 9.	Moheim		90	12	102	82	57
				Average	115	79·9	

The temperature of the water and air at Bahadurghur is the mean of two observations taken at an interval of fourteen days.

Respecting the last table Mr. Everest says, the irregularities in the temperatures may be explained by some of the wells being worked for the purposes of irrigation, and therefore supplied by a current of fresh water continually issuing from the ground; whilst in those wells which were still, the surface of stagnant water exposed to the air was cooled by the evaporation dependent on so dry a climate.

The general level of the country is said to be 800 feet above the sea, and the temperature of Delhi, as determined by the author from observations made during four years, to be 75°·78, or 74°·64 Fahr., taken as the mean between that result and another (73°·5) given in the 'Gleanings of Science.' If to this 1°·8 were added for the depth of the wells (115 feet), according to the rule which holds good in Europe, the temperature would be 76°·44, or something less than that obtained by observation. Mr. Everest then proceeds to show, according to the formula of Mr. Atkinson*, that the temperature at the level of the sea, in the latitude of Delhi (28° 40′), should be 77°·84 (74°·64 + 3°·2 for the difference of 800 feet of altitude), and that the temperature of Sincapore, in the second degree of north latitude, is 80°·2, leaving, in comparison with that of the former locality, only 2°·56 of temperature for above 26° of latitude. This discrepancy, he is of opinion, may be partly explained by Sincapore being surrounded by the sea contiguous to the Pacific and Indian Oceans, and cooled by perpetual showers, and Delhi being in the midst of dry and burning plains. But, adds the author, the mean annual temperature of Cairo, in 30° north latitude, and situated in a dry and sandy continent, is not above 72°·5, leaving consequently yet a difference to be accounted for, and which he conceives may be owing to the tropical rains being limited west of the Indus to 23½° north latitude, but extending in India even beyond 30° of latitude. During the period in which the rain prevails, or from the 25th of June to the 15th of September, the south-west monsoon blows nearly from the equator and transports a large quantity of aqueous vapour having a temperature from 77° to 81°, or that of the rain as it falls and soaks into the earth, the evaporation being then very trifling. The quantity of rain during the other nine months is so small that it cannot counteract this effect, which, Mr. Everest says, may account both for the high temperature of the surface, and for the temperature of the interior being greater than was to be expected.

* Transactions of the Astronomical Society for 1826, vol. ii. p. 137 *et seq.*

If this explanation be allowed, the author observes, it may easily be conceived that when a much greater portion of the globe was covered with water, and the evaporating surface consequently larger, currents of air charged with aqueous vapour prevailed still more, and modified the ancient climate even in still higher latitudes.

In conclusion, Mr. Everest remarks, that Scandinavia presents another instance of the carrying power of fluids with respect to heat, the coast, and even the bays, being free from ice to the latitude of 71°, owing probably to a south-westerly current in the adjacent ocean; and he states, on the authority of persons who have wintered at Spitzbergen, that south-west winds are usually accompanied by rain and thaw even in December and January.

A paper was then read, "On the Tertiary Formations and their connection with the Chalk in Virginia and other parts of the United States," by Charles Lyell, Esq., V.P.G.S.*

Having examined the most important cretaceous deposits in New Jersey, Mr. Lyell proceeded, in the autumn of 1841, to investigate the tertiary strata of Virginia, the Carolinas and Georgia, with a view to satisfy himself, first, how far the leading divisions of the tertiary strata along the Atlantic border of the United States agree in aspect and organic contents with those of Europe; and, secondly, to ascertain whether any rocks containing fossils of a character intermediate between those of the cretaceous and the eocene beds really exist. The conclusions at which he arrived, from his extensive survey, are given briefly as follows:—1. The only tertiary formations, which the author saw, agree well in their zoological types with the eocene and miocene beds of England and France; 2. he found no secondary fossils in those rocks which have been called upper secondary, and supposed to constitute a link between the cretaceous and tertiary formations.

1. *Virginia.*—The tertiary strata bordering the James River, Mr. Lyell says, have been well described by Prof. H. D. Rogers and Dr. Rogers†; and, he adds, they are also noticed in Mr. Conrad's excellent work on the tertiary strata of the United States. At Richmond, Mr. Lyell examined the remarkable bed of infusorial clay described by Prof. Rogers‡, consisting of an impalpable siliceous powder derived from cases of microscopic animalcules. It varies in thickness from twelve to twenty-five feet, and is interposed between eocene greensands and miocene clays; but Mr. Lyell agrees with Prof. Rogers in considering it as probably belonging to the former epoch.

Similar eocene greensands, very much resembling the cretaceous greensand of New Jersey, occur at Petersburg, thirty miles south of Richmond, and are overlaid by a large deposit of miocene marls abounding in testacea different from those of the subjacent sands. Among the fossils of the latter deposit are a Venericardia scarcely distinguishable from *V. planicosta* of the London clay, also an Ostrea

* For abstracts of a series of papers by Mr. Lyell and others on the geology of North America, see present volume, p. 180.

† American Phil. Trans., New Series, vol. v. p. 319 *et seq.* 1835, and vol. vi. p. 347 *et seq.* 1839.

‡ States' Report, 1840.

almost eqnally near to the *Ostrea bellovacina* and the *O. sellæformis*, so widely disseminated through the cocene formations of South Carolina and Georgia, is found in the uppermost beds of the formation at Coggin's Point, on the James River. The part of Virginia to which these remarks refer, is a flat region, forty or fifty feet above the level of the sea. The miocene strata which compose the upper beds consist sometimes almost exclusively of shells, and in the neighbourhood of Williamsburg Mr. Lyell collected eighty species, which bear a great resemblance generically, and in their relative numerical force, to collections from the Suffolk crag and the Faluns of Touraine. Among these Testacea are several species of Astarte, some very analogous to those of Suffolk, the *Voluta mutabilis*, which resembles the *V. Lamberti*, also *Conus diluvianus*, *Lucina squamosa*, and *L. divaricata*. Mr. Lyell says there are many other analogies among the Mollusca, besides the occurrence of several corals, Echinodermata, fishes' teeth and bones of Cetacea; but he shows that the most important point of comparison is in the proportion of recent to extinct Testacea. Out of eighty-two species which he collected at Williamsburg, sixteen are considered by Mr. Conrad to be recent, and found for the greater part living on the coasts of the United States. The existing species, therefore, are in the proportion of one-fifth of the whole, which agrees well, says the author, with the average per-centage in the shells obtained by him in 1840 from the Faluns of Touraine. The entire number of American miocene shells known to Mr. Conrad is 238, of which thirty-eight have been identified with recent species.

North Carolina.—In the neighbourhood of South Washington, on the north-east branch of Cape Fear river, Mr. Lyell found the dark, bluish marls of the cretaceous series, to which his attention had been directed by Mr. Hodge's paper in Silliman's Journal*. They closely resemble, in composition and organic contents, those in New Jersey, and abound with *Belemnites mucronatus*, *Exogyra costata*, and a species of Gryphæa resembling *G. columba*; Mr. Lyell also found in them *Ostrea vesicularis* and *O. pusilla* of Nillson, likewise *Anomia tellinoides*, a species of Plagiostoma, and several new shells. These marls extend to the south of Lewis Creek, for several miles along the banks of the north-east branch of Cape Fear river, nearly to Rocky Point, where they are covered by the Wilmington limestone and conglomerate. This formation, which is overlaid by miocene strata, and ranges to Wilmington, as well as along the coast to Cape Fear river, has been considered by Mr. Hodge, and other geologists, to be an upper secondary deposit, or interposed between the eocene and cretaceous series; but Mr. Lyell could find in it no organic forms which supported this opinion, nor could he learn that any had been discovered. On the contrary, the only determinable species apparently agree with the *Lucina pendata*, an Alabama shell, and *Pecten membranosus*, both eocene fossils. The organic remains at Wilmington are only casts, but are referable to the genera Cardium, Nucula, Corbula, Cardita, Venus, Arca, Natica, Oliva, Cypræa, Conus, Calyptræa, and Siliquaria. Associated with these remains

* Vol. xli. p. 332, 1841.

of Testacea are a species of Lunulites and several other corals, the claws of Crustacea, and teeth of the Lamna family. Many of these fossils occur at Rocky Point, including *Pecten membranosus*, with a Lunulite and a Vermetus subsequently found by the author in the limestone of the Santee canal in South Carolina.

South Carolina and Georgia.—Charleston stands on a yellow sand, beneath which is a blue clay containing the remains of Testacea that inhabit the adjacent seas; and Dr. Ravenel informed Mr. Lyell that he had found in it the *Gnathodon cyrenoides*, not now known to occur in a living state nearer than the Gulf of Mexico. The author could not ascertain whether this post-pliocene formation rises above high-water mark; but he states that, on the Cooper river thirty miles north of Charleston, there occurs beneath the superficial sand and mottled clay a freshwater formation, in which Dr. Ravenel has found the remains of the Cypress, Hiccory and Cedar, which must have grown in a freshwater swamp, although the formation is now six feet below the level of high water. No shells have been noticed in the deposit, but they are also commonly wanting in the marsh accumulations of that region. As the salt water of Cooper river must now cover much of this deposit, a very modern subsidence, Mr. Lyell says, must have taken place along the coast. At Dr. Ravenel's plantation in the low country near the mouth of Cooper river is a pulverulent limestone, artificially exposed, which Mr. Lyell thinks may be an eocene formation, though its fossils differ from those of other deposits of that epoch.

Between this point and Vance's Ferry, on the Santee river, is a continuous formation of white limestone, which Mr. Lyell examined with Dr. Ravenel at Strawberry Ferry, Mulberry Landing, the banks of the Santee canal, Wantout and Eutaw. It varies in hardness, and consists of comminuted shells; but it very rarely exhibits any laminæ of deposition, and even where it attains a thickness of twenty or thirty feet there would be a difficulty in determining whether it were horizontal, if a bed of oysters, like that at Vance's Ferry, did not occasionally occur. At the Rock bridge near Eutaw springs, the limestone composed of comminuted shells, corals, the spines of Echini, &c., resembles so precisely the upper cretaceous formations at Timber Creek in New Jersey, that Mr. Lyell at first felt no doubt of the identity of the two formations, although the organic contents of the limestone prove that it belongs to the tertiary series. This resemblance has led to the admission into Dr. Morton's excellent work on the fossils of the cretaceous group, of the *Balanus peregrinus*, *Pecten calvatus*, *P. membranosus*, *Terebratula lachryma*, *Conus gyratus*, *Scutella Lyelli*, and *Echinus infulatus* *, though they do not really belong to the chalk series; and to several other similar mistakes, whereby, Mr. Lyell observes, beds of passage have been erroneously supposed to exist. Among the most widely distributed of the limestone fossils is the *Ostrea sellæformis*; and he searched in vain at various points throughout a distance of forty miles for an admixture of characteristic cretaceous and tertiary organic remains, though the

* See pl. 10. of Morton's Synopsis.

chalk formation, containing Belemnites and Exogyræ, occurs between Vance's Ferry and Camden. The Santee limestone, he is of opinion, cannot be less than 120 feet thick at Strawberry Ferry, being vertically exposed to the extent of seventy feet in the banks and bottom of Cooper river, and to the height of fifty feet in the neighbouring hills. Its upper surface is very irregular, and is usually covered with sand in which no shells have been found. Mr. Lyell followed the limestone north-westwardly for twelve miles by Cave Hall and Struble's Mill to near Half-way Swamp. At Stoudenmire or Stout Creek, a tributary of the Santee, it has disappeared beneath a newer tertiary deposit of considerable thickness, consisting of slaty clays, quartzose sand, loam of a brick-red colour, and beds of siliceous burr-stone. Mr. Lyell is not aware of any published description of this formation, though he afterwards met with it on the Savannah river. In both localities some of the clays break with a conchoidal flinty fracture when dry, and even occasionally pass into a stone closely resembling menilite. The fossils which he found were in the state of casts. He does not determine whether this formation should be regarded as an upper division of the eocene group or not; but he has little doubt that it is of the same age as the burr-stone series of Georgia. In the notice of the cretaceous and tertiary strata of the Southern states, drawn up by Dr. Morton from the notes of Mr. Vanuxem, the tertiary limestone and the burr-stone sand and clay are included in the same group, and Mr. Vanuxem informed Mr. Lyell that he had not been able to determine their relative position; but from what Mr. Lyell saw on the Savannah river, he infers that the burr-stone formation is above the limestone. One of the strata at Stoudenmire is extremely light and of white colour and resembles calcareous tufa, but according to the analysis of Prof. Shepard it contains no carbonate of lime; Mr. Lyell, therefore, states it may probably be of infusorial origin.

At Aikin, sixty miles west of Orangeburg, and near the left bank of the Savannah, an inclined plane in a railway has been cut through strata 160 feet in thickness, consisting of earth and sand of a vermilion colour and containing much oxide of iron; also of mottled clays and white quartzose sand with masses of pure white kaolin. These strata are within ten miles of the junction of the tertiary formation and the great hypogene region of the Appalachian or Alleghany chain, and their materials, Mr. Lyell states, have evidently been derived from the decomposition of clay-slate and granitic rocks. No fossils were observed by him in the deposit at Aikin. A similar formation is extensively developed at Augusta, where the Savannah divides the states of South Carolina and Georgia, and it must, in some places, be more than 200 feet thick. Three miles above the town are the rapids, which descend over highly inclined clay-slate and chlorite schist, overlaid unconformably by tertiary beds. This point is the western boundary of the supracretaceous series; and Mr. Lyell observes, that on all the great rivers of the Atlantic border from Maryland to Georgia, and still further south, the first falls or rapids are along a line at which the granitic and hypogene rocks meet the tertiary, and which

X 2

is nearly parallel to the Atlantic coast, but at the distance of 100 or 150 geographical miles. This great feature, Mr. Lyell states, was first pointed out by Maclure, but he adds that portions of the tertiary formations usually cover the hypogene rocks for a certain distance above the Falls, and that their outline is very irregular and sinuous. On Race's Creek near Augusta, the highly inclined clay slate, containing chloritic quartzose beds with subordinate strata much charged with iron, are decomposed to the depth of many yards into clays and sands which resemble so precisely a large portion of the horizontal tertiary strata of the neighbouring country, that the disintegrated materials might be mistaken for them, if the veins of quartz which often traverse the argillaceous beds at a considerable angle, did not continue unaltered. The only point at which Mr. Lyell saw any organic remains in beds associated with these upper tertiary red strata was at Richmond in Virginia, where he obtained casts of decidedly miocene fossils; but as he observed on the Savannah river thick beds of sandy-red earth beneath the burr-stone of Stony Bluff, he concludes that the same mineral character may sometimes belong to the upper division of the eocene group. At the rocks six miles west of Augusta, the tertiary beds derived from the hypogene rocks have the appearance of granite, and have been called gneiss by some geologists. They exhibit occasionally a distinct cross-stratification, and include angular masses of pure kaolin.

Though the Savannah, in its course from Augusta to the sea, flows for the greater part in a wide alluvial plain, and has a fall of less than one foot in a mile, yet Mr. Lyell descended it to obtain information, by means of the Bluffs, respecting the superposition of the several masses, natural sections being otherwise difficult to obtain. After passing cliffs of horizontal strata in which the brick-red sand and loam prevail, the first exposure of a new deposit was observed at Shell Bluff, forty miles below Augusta. The height of the section was 120 feet, and its extent more than half a mile. The lowest exposed strata consisted of white, highly calcareous sand, derived chiefly from comminuted shells, but the beds passed upwards into a solid limestone, sometimes concretionary, and containing numerous casts of shells. In one place a layer of pale green clay showed the horizontal character of the formation. The upper part of this deposit is more sandy and clayey, and incloses a bed of huge oysters, *Ostrea Georgiana*, occupying evidently the position in which they lived. The total thickness of these lower strata is eighty feet. The upper portion of the cliff is composed of forty feet of the red loam which prevails at Aikin and Augusta, and yellow sand. Mr. Lyell did not find any fossils in this deposit, but he believes that it belongs to the burr-stone formation, and therefore to be an upper eocene accumulation. At his first inspection of the casts contained in the limestone, he inferred that they belonged to eocene species, without any intermixture of cretaceous or miocene forms; but it was not till he had the advantage of Mr. Conrad's assistance that he was able to determine the following twelve species which are well known to be characteristic fossils of the eocene beds of Claiborne and Alabama:—

Oliva Alabamiensis.	Corbula nasuta.
Calyptræa trochiformis.	——— oniscus.
Dentalium alternans.	Nucula magnifica.
Venericardia planicosta.	Crassatella prætexta.
Cytherea Poulsoni.	Ostrea sellæformis.
——— perovata.	——— Alabamiensis.

The same shelly, white, calcareous beds, overlaid by red clay and loam, are exhibited at London Bluff, nine miles below Shell Bluff, and a horizontal bed of the large oysters is exposed in a cliff two miles farther down the river. At Stony Bluff, on the borders of Scriven county, the calcareous deposit is no longer visible, the cliff being composed of siliceous beds of the burr-stone and millstone series, resting upon brick-red and vermilion-coloured loam. This section, Mr. Lyell states, is of great importance, as it concurs in proving that the millstone of this region, with its eocene fossils, is an integral part of the great red loam and sand formation usually devoid of organic remains. The burr-rock of Stony Bluff abounds with cavities and geodes partially filled with crystals of quartz and agates. In the fragments scattered over the adjacent fields Mr. Lyell observed casts of univalves. At Millhaven, eight miles from Stony Bluff and five from the Savannah river, these siliceous beds again crop out and afford casts of the genera Pecten, Eulima or Bonellia, and a Cidaris. It had been pierced through to the depth of twenty-six feet, and was associated with red loam, white sand and kaolin, affording further evidence of these deposits belonging to one formation.

One mile west of Jacksonborough, in the ford of Briar and Beaver Dam Creeks, is a limestone passing upwards into white marl which appears to have been deeply denudated, and is overlaid by sand that belongs to a formation of sand, loam, and ferruginous sand-rock, referred by Mr. Lyell to the red loam and burr-stone series. The limestone and marl, although rarely exposed in sections, are considered to constitute very generally the fundamental strata of the region on account of the not unfrequent occurrence of lime-sinks or circular depressions, formed in the beds of loam and sand by subterraneun drainage. The fossils procured from the limestone of Jacksonborough by Mr. Lyell, as well as those presented to him by Col. Jones of Millhaven, were for the greater part well-defined casts, and were specifically new to American palæontologists; nevertheless he has no hesitation, from their general aspect, to regard them as belonging to the eocene period. The genera enumerated in the paper are, Conus, Oliva, Bulla, Voluta, Buccinum, Fusus, Cerithium?, Trochus, Calyptræa, Dentalium, Crassatella, Chama, Cardium, Cytherea, Lithodomus, Lucina, Pecten, and Ostrea. The Trochus is considered identical with the *T. agglutinans* which occurs in the Paris basin; and the Lithodomus to be undistinguishable from the *L. dactylus* of the West Indies, one of the few eocene Parisian fossils identified by Deshayes.

All the Bluffs examined by Mr. Lyell on the Savannah river below Briar Creek belong to the beds above the limestone, and are referable chiefly, if not entirely, to the burr-stone formation. In white clays exposed a few hundred yards below Tiger Leap in Hudson's

Reach, the author found impressions of Mactra, Pecten and Cardita, also fragments of fishes' teeth, particularly of the genus Myliobates, likewise several teeth of the genus Lamna, and one belonging to a Notidamus or a nearly allied genus. At Sisters Ferry he observed not only the brick-red loam, with the red and grey clay and sand, but a highly siliceous clay, which though soft when moist, exhibits a conchoidal fracture when dry, and resembles flint; in some spots the clay also passes into a kind of menilite.

In conclusion, Mr. Lyell offers the following general observations. The part of South Carolina and Georgia which lies between the mountains and the Atlantic, and of which he examined a portion near the Santee and Savannah rivers, has a foundation of cretaceous rocks containing Belemnites, Exogyræ, &c., overlaid first by the eocene limestone and marls, and secondly by the burr-stone formation with the associated red loam, mottled clay, and yellow sand. According to Mr. Vanuxem's observations, a tertiary lignite deposit sometimes intervenes between the cretaceous and eocene series. The remarkable difference in the fossils of the eocene strata at different points, as the Grove on Cooper river, the Santee canal, Vance's Ferry, Shell Bluff, Jacksonborough, and Wilmington, might lead, Mr. Lyell states, to the suspicion of a considerable succession of minor divisions of the eocene period. That the whole are not precisely of the same age he is willing to believe, but he is inclined to ascribe the difference principally to two causes: 1st, that the number procured at each place is small and therefore represents only a fractional portion of the entire fauna of the period, so that variations in each locality may have arisen from original geographical circumstances; and 2ndly, no great eocene collection has been made from any part of the United States.

Some of the burr-stone fossils occur in the limestone, and Mr. Lyell thinks the former may bear to the latter a relation analogous to that which the upper marine sands of the Paris basin bear to the calcaire grossier.

With respect to the conclusion stated in the beginning of the paper, that he had been unable to find any beds containing an intermixture of cretaceous and tertiary fossils, Mr. Lyell says, it would require far more extended investigations to enable a geologist to declare whether there exist in the Southern states any beds of passage, but he affirms that the facts at present ascertained will not bear out such a conclusion.

The generic affinity of the cretaceous fossils of the United States to those of Europe is stated to be most striking, and Mr. Lyell observed in Mr. Conrad's collection from Alabama a large Hippurite, a point of analogy not previously recorded.

The proportion of recent shells in the eocene strata of the United States appears to be as minute as in Europe, and the distinctness of the eocene and miocene testacea hitherto observed to be as great. Mr. Lyell says, it is also worthy of remark, that the recent shells found in the American miocene beds are not only in the same proportion to the extinct as those of the Suffolk crag, or the Faluns of Touraine, but that they also agree specifically in most cases with mollusca inhabiting the neighbouring sea; in the same manner as the recent

miocene species of Touraine agree for the greater part with species now living on the western coast of France or in the Mediterranean, and as the recent testacea of the crag are identifiable with species belonging to the British seas. This result appears to Mr. Lyell to confirm the accuracy of conchological determinations; for if, on the contrary, it should be maintained, that the number of recent species is so enormous, and different species resemble each other so closely as to have produced identifications from the mere difficulty of effecting discriminations, he would suggest that in that case, according to a fair calculation of chances, nine-tenths of the American miocene species hitherto identified ought to have been assimilated to exotic shells, instead of having been found to agree with some portions of the limited fauna at present known on the American shores. The same argument, he adds, is clearly applicable to the identifications which have been made of fossil and recent shells in the European tertiary formations.

May 18, 1842. A memoir "On the Geological Structure of the Ural Mountains," by Roderick Impey Murchison, Esq., F.R.S., Pres. G.S., Mons. E. de Verneuil, and Count A. von Keyserling, was read; an abstract of which has been given in the present volume, p. 124.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 154.]

June 9, 1843. (Communications respecting the Comet concluded.) An article by M. Capocci, on the comet, of which the following is an abstract, is extracted from the *Giornale della due Sicilie*, of 1st May, 1843, and communicated by Colonel Jackson.

The article gives an account of a paper read by M. Capocci before the Royal Academy of Sciences of Naples. M. Capocci first corrects a mistake into which some observers appear to have fallen, in over-estimating the length of the tail, to which some persons attributed an extent of 80° to 90°, but which certainly was not visible beyond 40° to 45° from the nucleus. With respect to the difficulty attending the orbit of the comet, he attributes it to the very small perihelion distance, and the consequently very rapid angular motion at the passage through the perihelion; the comet, during the eighteen days following its perihelion passage (that is, prior to the time of its first observation on March 17), having gone through at least 170° of its angular motion round the sun; while, during the whole of the time of its visibility afterwards, it described only 3°, from which the orbit was to be determined; whence it has happened that astronomers of very high reputation have published results altogether false. With respect to the particular difficulty attending the circumstance of some of the sets of observations having given a perihelion distance smaller than the sun's semi-diameter, and the apparent consequence that the comet must thus either have passed within the luminous matter of the sun, or have been projected obliquely from his surface, M. Capocci considers that it is more seeming than real, as an error sufficient to account for such a paradox would have excited no surprise in an orbit with a greater perihelion distance.

In the meanwhile, the parabolic orbit, which seems to represent est all the observations, is the following:—

Perihelion Passage, Feb. 27·5643.

Perihelion Distance..........	0·00538
Long. of the Perihelion......	277° 52′ 35″
Long. of the Node..........	354 48 50
Inclination................	35 56 55

Motion retrograde.

M. Capocci thinks it probable, however, that the comet really moves in an elliptic orbit, and that it has appeared several times previously. He thinks it exceedingly probable that the comets of 1618, 1668 and 1702, were identical with the one in question, and that that of 1689 was still more clearly so, a probability which has not suggested itself to any one on account of the orbit of that comet inserted in the catalogue, calculated by Pingré, not being correct. But M. Capocci has found that, supposing the day of the perihelion passage in the year 1689 to have been December 3, the old observations of that comet are sufficiently well represented by the elements of the present one. The physical characters of the comet coincide also perfectly with those of the present one. Now this new and undeniable recognition, observes M. Capocci, curiously modifies the supposed period; and to make it satisfy all the returns of which we have an account, it is perhaps necessary to reduce it to one of seven years nearly. He does not deny the difficulty of explaining how it has happened that the comet has not been seen at its nineteen former returns; but he contends that it is less difficult to do this than to account for the strange coincidence in the positions and in the physical appearances of the four comets above mentioned. The following is the whole series of the apparitions which may possibly belong to this one and the same body:—

1618, 1652, 1668, 1689, 1702, 1723, 1758, 1843.

Without laying very great stress on this coincidence, he thinks it proper to draw the attention of other astronomers to it, to the end that each, deducing a corresponding ellipse from his own observations, may either confirm or destroy the hypothesis; a circumstance so much the more important, as each may cherish the reasonable hope of seeing with his own eyes, within the space of seven years, the prediction verified.

The following is an abstract of a notice of the comet from a Madras paper received by the Astronomer Royal:—

"The comet was first seen on the 2nd of March, but the only part seen above the horizon was part of the tail, and that faintly.

"On the 3rd and 4th the nucleus was distinctly visible to the naked eye: the tail was divided into two distinct branches, the one long, but faint, the other much shorter, but broader and much brighter.

"On the 5th the tails had apparently united; but on a careful examination a less luminous band was detected between them.

"On the 6th several stars were visible through the tail, which near the star τ Ceti was about 40′ in breadth. At this part it appeared through the telescope to consist of three luminous bands; the one next to the sun being broad and bright, the other two fainter

and more narrow towards the nucleus. These bands were less distinct, and not more than a single separation could be detected. The nucleus appeared like a star of the fourth or fifth magnitude: its light was pale, and it was surrounded by a luminous halo of no great extent."

Observations of the Comet made at the Observatory of Trevandrum, accompanied by a Drawing. By J. Caldecott, Esq., Director of the Observatory.

The observations were made with an achromatic telescope of $7\frac{1}{2}$ feet focal length and 5 inches aperture, made by Dollond for the Observatory. It is mounted equatoreally on exactly the same plan as Mr. Bishop's instrument, the ends of the polar axis (which is of brass) being supported on pillars of granite. The micrometer made use of is a reticulated diaphragm of gold wire. The instrument keeps its adjustments very permanently, and the place of a known star (after correction for collimation and index error) seldom differs more than a second of time in right ascension, and 15″ to 20″ in declination.

The right ascensions and declinations of the comet are those read from the circles, after being corrected for instrumental errors, and for the effects of refraction, the instrumental corrections having been obtained almost every evening by observations of θ Ceti, when at nearly the same hour-angle as the comet was observed afterwards. In addition, differential observations of small stars passing through the field within a few minutes before or after the comet have been obtained, and the results will be communicated after the places of the stars have been determined by meridional observations.

The following is Mr. Caldecott's account of the observations:—

Places of the Comet.

Trevandrum Observatory, Lat. 8° 30′ 32″ N.; Long. $5^h\ 7^m\ 59^s$ East.

Date.	Trevandrum Mean Time.	Observed Right Ascension.	Observed North P. D.	Remarks.
1843.	h m s	h m s	° ′ ″	
March 6.	7 4 35·30	0 33 56·4	101 58 0	The N. P. D. is probably erroneous this evening on account of interruption from visitors.
7.	Observations	prevented by	clouds.	
8.	6 54 30·81	1 0 45·0	102 7 22	The corrections obtained from β Ceti.
9.	6 48 27·13	1 13 48·7	102 0 44	
10.	6 50 47·96	1 26 22·1	101 51 37	Ditto from θ Ceti.
11.	6 43 53·35	1 38 19·4	101 41 47	Ditto ditto.
12.	Observations	prevented by	clouds & rain.	
13.	7 5 57·15	2 0 31·2	101 15 20	Ditto ditto.
14.	6 53 36·38	2 10 37·8	101 0 6	Ditto ditto.
15.	7 13 2·85	2 20 21·3	100 43 22	Ditto ditto.
16.	6 45 57·44	2 29 20·6	100 27 4	Ditto ditto.
17.	6 45 19·51	2 37 57·0	100 9 48	Ditto ditto.
18.	6 59 30·01	2 46 11·7	99 52 2	Ditto ditto.
19.	7 11 56·56	2 53 59·7	99 34 43	Ditto ditto.
20.	Not observed	on account of	clouds.	

Notes.—The comet was first seen (partially only) on the 4th of March, about half-past six p.m.; but clouds over the head of it, which was besides very near the horizon, prevented any observations.

On the 5th a larger portion of the tail was visible, and it was evidently higher than the evening before; clouds, however, again hung over the head until it set.

On the 6th the sky was free from clouds, and the comet presented a most magnificent appearance. Observations of it in Right Ascension and North Polar Distance were obtained this evening with the equatoreal; but from the excitement at first view of so splendid an object, together with the confusion caused by a number of visitors at the Observatory, I do not consider them entitled to much confidence, especially those in North Polar Distance. The length of the tail I measured roughly with a sextant, by bringing down the image of a star which happened to be situated near the faint end of it, into contact with the head, and made it to be about 36°; but from a much better measurement made in the same way on the 13th, this was probably too small. The nucleus of the head (seen through the 7½-feet telescope) presented rather a well-defined planet-like disc, the diameter of which I *estimated* to be about 12″, and that of the nebulosity surrounding it at about 45″. The tail had a dark appearance along its axis as if hollow; and at about half-way from the head, it even appeared to separate slightly into two parts, the upper one being rather longer than the other.

On the 13th, after the observations for position, I introduced a parallel wire micrometer, with a view to measure the diameter of the bright part, or disc, of the head, and, by a pretty fair measure, made it to be 11″. The nebulosity about it I *estimated* to be about four times the diameter of the bright part. The length of the tail, measured carefully with a sextant, I found to be 45°; its breadth, at one-third its length from the head, 33′, and at two-thirds its length, 60′.

Since the 19th the weather has been unfavourable, and no observations have been obtainable. The comet appears to be getting somewhat fainter than it was on the evenings of the 6th and 8th, but only slightly, and very slowly so.

Trevandrum Observatory, March 22, 1843. JOHN CALDECOTT.

A second letter has been received from Mr. Caldecott, dated April 21, giving the following additional observation:—

$$\text{March 26.}\ 7^{h}\ 3^{m}\ 36^{s}{\cdot}35\ \text{Trev. M. T.}\quad \text{R. A.} = 3^{h}\ 38^{m}\ 7^{s}{\cdot}3.$$
$$\text{N. P. D.} = 97°\ 39'\ 12''.$$

From the observations of the 8th, 13th, and 18th of March, Mr. Caldecott computed the parabolic elements, which are as follow:—

Long. of the Ascending Node	3° 7′
Inclination	35 3
Long. of the Perihelion	279 6
Perihelion Distance	0·0048

Time of Perihelion Passage, Feb. 27·654, Trevandrum mean time. Motion retrograde.

XXXVII. *Intelligence and Miscellaneous Articles.*

ON THE NON-PRECIPITATION OF LEAD FROM SOLUTION IN SULPHURIC ACID BY HYDROSULPHURIC ACID. BY. M. DUPASQUIER.

WHEN a current of hydrosulphuric acid is passed through, or an aqueous solution of this acid gas is poured into, commercial sulphuric acid diluted with an equal weight of water, only tin and

arsenic, if they be present, are precipitated; and the precipitate contains no sulphuret of lead. As to the iron which the sulphuric acid contains, it is well known to be the protosulphate, upon which hydrosulphuric acid has no action.

The non-formation of sulphuret of lead in this case had led the author to think, contrary to the general opinion, that commercial sulphuric acid does not contain sulphate of lead, and consequently that this metal is completely insoluble in it; but on trial he adopted a contrary opinion. The following experiments were performed:—

1. Recently precipitated sulphate of lead was put into a glass and covered with concentrated sulphuric acid, and exposed to the air during about six months, taking care to shake the mixture occasionally. The acid was considerably diluted by absorbing atmospheric moisture. This acid, rendered clear by standing, was submitted to the action of a current of hydrosulphuric acid gas without occasioning any discoloration or precipitation of sulphuret of lead.

2. Sulphuric acid of sp. gr. about 1·540, was boiled for an hour on sulphate of lead, and afterwards the experiment was repeated with concentrated acid. The liquids rendered clear by standing were treated with a current of hydrosulphuric acid gas, but neither precipitation of sulphuret of lead nor discoloration were produced.

These experiments seem to prove that even boiling concentrated sulphuric acid does not dissolve sulphate of lead, and consequently that the acid of commerce cannot contain any; but on adding water to the acids which had been boiled with the sulphate of lead, after they had become clear, a considerable white precipitate was formed; this could only be attributed to the separation of the acid from the sulphate of lead which it had dissolved, an effect which is precisely similar to the precipitation of sulphate of barytes dissolved by concentrated sulphuric acid.

An aqueous solution of hydrosulphuric acid was then added to the acid which had been treated with water, and still holding in suspension the white precipitate which had been formed; but neither the liquid nor the precipitate was rendered brown by the hydrosulphuric acid: they remained perfectly colourless. From these facts M. Dupasquier began to suspect that sulphuric acid prevented the formation of sulphuret of lead; that this is actually the case was proved by the following experiment:—

Sulphate of lead was put into a glass and covered to about 1½ inch of concentrated sulphuric acid, agitation being used to effect their mixture. Being afterwards subjected to the action of hydrosulphuric acid, both in its gaseous state and in solution, the mixture remained perfectly white. The same result was obtained by causing hydrosulphuric acid to react upon sulphuric acid, which had been boiled with sulphate of lead, and then mixed with this salt; in neither case was there the slightest formation of sulphuret of lead.

In order to prove that the discoloration both of the dissolved and undissolved sulphate of lead was owing to the presence of an excess of sulphuric acid, the following experiments were performed:—

1. The precipitated sulphate of lead was washed with distilled

water, and treated with hydrosulphuric acid, when it became immediately black.

2. Sulphuric acid which had been boiled with sulphate of lead was saturated with potash; in this state a current of hydrosulphuric acid immediately rendered it black, and on standing a deposit of sulphuret of lead was formed.

It follows from what has been stated,—

1st. That a small portion of sulphate of lead is soluble in concentrated sulphuric acid.

2nd. That hydrosulphuric acid does not react upon sulphate of lead dissolved in a great excess of sulphuric acid, or mechanically mixed with it.

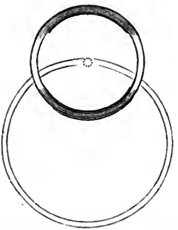
3rd. That consequently, hydrosulphuric acid cannot be employed for the purpose of ascertaining the presence of sulphate of lead in commercial sulphuric acid.

4th. That boiling concentrated sulphuric acid dissolves some sulphate of lead, the greater part of which is precipitated on the addition of water.

5th. That hydrosulphuric acid immediately reacts, and sulphuret of lead is instantly formed from the sulphate whether it is dissolved or not, when the excess of sulphuric acid is saturated by an alkaline base; from which it evidently results, that it is the excess of sulphuric acid that prevents the reaction of the hydrosulphuric acid on the oxide of the sulphate of lead.—*Journal de Pharmacie et de Chimie*, Août, 1843.

HALO ROUND THE SUN, SEEN BY MR. VEALL, BOSTON.

S.W.



At Boston, June 16th, 1843, at 2^h 30^m p.m., was seen a halo round the sun, with prismatic colours on the north-east and south-

west, and a much larger circle, well-defined, of a pale white, having the sun in the south-west of its circumference.

The interior of the halo, except the sun's disc, was of a much darker colour than the surrounding atmosphere.

The centre of the larger halo was very near, if not in the zenith.

CRYSTALLIZATION OF OCTAHEDRAL IODIDE OF POTASSIUM. BY M. BOUCHARDAT.

By evaporating a saline solution containing iodine, iodide of potassium and acetic æther, M. Bouchardat obtained light yellow-coloured semitransparent octahedral crystals. These crystals, when heated in a tube, yielded traces of iodine, and the fused residue consisted entirely of iodide of potassium; similar crystals were produced from a solution of biniodide of potassium by spontaneous evaporation; in order to obtain them there must be a great excess of iodine in the solution, although they do not contain 1-1000dth of their weight of free iodine; but it is certainly curious to observe the iodide of potassium lose its usual form owing to the presence of so small and indefinite a portion of iodine.—*Journal de Pharm. et de Chim.*, Juillet 1843.

ON THE PRESENCE OF THE SULPHATE OF TIN IN THE SULPHURIC ACID OF COMMERCE. BY M. DUPASQUIER.

It is generally known that the sulphuric acids of commerce contain lead, iron, and frequently arsenic; but I am not aware that the existence of tin in them has hitherto been noticed. Nevertheless this metal may be obtained, and in somewhat considerable quantity, from most of the commercial acids; and it will not be useless to be aware of this circumstance, which may have some influence in many operations, especially in those of dyeing, which should be taken into consideration.

I found sulphate of tin in all the acids which I examined while engaged in the researches which I have published on the arseniferous sulphuric acids in the following manner:—In order to precipitate the arsenic of these acids, I diluted them with twice or six times their weight of water, and passed a current of sulphuretted hydrogen through them, which gave rise to a yellowish-brown precipitate when the acid contained arsenic; this precipitate was less considerable, and of a darker brown when the acid was not arseniferous.

Thinking that sulphuret of lead might have been formed, and that the brown colouring of the sulphuret of arsenic should be attributed to that compound, I treated the precipitates obtained by the action of sulphuretted hydrogen on the sulphuric acids with nitric acid, and I constantly obtained a white residue, insoluble in water, soluble in *aqua regia*, which solution presented all the characters of the nitro-muriate of tin. With respect to the solution effected by the nitric acid, I found it to be arsenic acid when this sulphuret of tin

was mixed with the sulphuret of arsenic. I could never detect a trace of lead, which circumstance will be accounted for in a subsequent notice.

Having always found sulphate of tin in the sulphuric acids submitted to examination, I questioned myself as to its origin, and I soon ascertained that it was simply due to the action which the acid has on the solder of the leaden chambers. Now it is well known that the soldered portions are very rapidly corroded by the acid vapour with which they are in constant contact.

The presence of tin in the sulphuric acid of commerce accounts for the traces of this metal which have sometimes been found in the green vitriol of commerce.—*Journ. de Pharm.* for August.

ON THE OXIDIZING ACTION OF CHLORATE OF POTASH ON NEUTRAL SUBSTANCES.

M. Barreswill has communicated to the 'Journal de Pharmacie' for August a very interesting fact which he had occasion to observe in conjunction with M. Köchlin, while investigating the mode of action of the chlorate of potash as an oxidizing agent.

When a hot solution of this salt is mixed with a solution of the protosulphate of iron, likewise hot, the two perfectly-transparent liquids immediately become turbid, and exhibit in suspension a considerable red precipitate. The filtered liquor is also of a red colour. The reaction is one of the most simple and most precise that can be imagined; the chlorate of potash loses the whole of its oxygen, which goes entirely to the protosulphate of iron, causing this to pass into the state of the persulphate, in part neutral salt and in part basic, without any perchlorate being formed:—

$$KO\,ClO^5 + 12FeO, SO^3 = KCl + 3Fe^2O^3, (SO^3)^3 \text{ (in solution)}$$
$$+ 3Fe^2O^3\,SO^3 \text{ (precipitated)}.$$

The same reaction takes place in the cold, but more slowly. At the boiling temperature it is complicated, from the action of the neutral sulphate of the peroxide of iron on the chlorate of potash, which may be compared to that of sulphuric acid; for, in fact, the neutral sulphate is converted into the subsulphate, and the two equivalents of acid react on the chlorate of potash. The subsulphate deposited from a hot solution is yellow, anhydrous, and dissolves with difficulty in acids, while the subsalt which subsides from a cold solution is red, hydrated, and is very soluble in dilute acids. All the neutral salts of the protoxide of iron behave in a similar manner, which indeed is the case with all neutral substances susceptible of oxidation by exposure to the atmosphere; the chlorate of potash abandons the whole of its oxygen to them.

Iron and zinc become oxidized in a solution of the chlorate, and soon the liquid contains chloride only; the action, which is somewhat energetic, is singularly diminished by the layer of oxide which forms and protects the metal.

Lead does not oxidize under the same circumstances, but if placed

at the same time in contact with water, chlorate and carbonic acid, without the air having access, it is gradually converted into white lead, a fact which very much confirms M. Pelouze's theory of the formation of this compound.

A solution of chlorate of potash in water is therefore a powerful oxidizing agent for neutral substances, abandoning both the oxygen of its acid and that of its base. Its action may be compared to that of air or weakly oxygenated water. This property will without doubt find numerous applications.—*Journ. de Pharm.* for August.

NEW BOOKS.

A Series of Tables of the Elementary and Compound Bodies, systematically arranged, and adapted as Tables of Equivalents, or as Chemical Labels. By Charles Button and Warren De la Rue. Part I.

A Memoir of the Life, Writings, and Mechanical Inventions of Edmund Cartwright, D.D., F.R.S., Inventor of the Power Loom, &c. &c.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1843.

Chiswick.—August 1. Very fine. 2. Cloudy and fine. 3. Cloudy: thunder-storm, with very heavy rain, the latter continuing throughout the night. 4. Rain: showery: clear. 5—8. Very fine. 9. Sultry: lightning at night. 10. Hazy: clear and fine. 11—14. Exceedingly fine. 15. Sultry: thunder-storm at night. 16. Thunder, lightning and heavy rain: clear and fine at night. 17. Foggy: sultry. 18. Foggy: hot and sultry: clear and fine. 19. Cloudless and very fine. 20. Overcast and fine. 21. Clear: cloudy and fine: clear. 22. Overcast: rain. 23. Fine: overcast: heavy rain at night. 24. Cloudy: clear and fine. 25. Very fine: cloudy: lightning. 26, 27. Very fine. 28. Rain: overcast and windy. 29. Cloudy. 30. Light haze and fine. 31. Hazy: very fine: clear.—Mean temperature of the month 1°·1 above the average.

Boston.—Aug. 1. Cloudy. 2. Fine. 3. Fine: rain A.M. and P.M. 4. Cloudy: rain A.M. and P.M. 5. Fine. 6. Fine: rain early A.M. 7. Cloudy. 8. Fine: thermometer 77° 2 o'clock P.M. 9. Cloudy: rain, thunder and lightning from 11 A.M. to 11 P.M. 10. Cloudy. 11—13. Fine. 14. Fine: rain, thunder and lightning at night. 15. Rain: heavy thunder-storm A.M. 16. Cloudy: heavy rain P.M. 17. Cloudy. 18. Foggy. 19. Fine. 20. Cloudy: rain P.M. with thunder and lightning. 21. Fine. 22. Cloudy: rain P.M. 23. Fine. 24. Rain: rain early A.M. 25—28. Fine. 29. Cloudy: rain early A.M.: rain A.M. 30, 31. Cloudy.—N.B. This month shows the largest fall of rain in one month since July 1839.

Sandwick Manse, Orkney.—Aug. 1. Cloudy: rain. 2. Cloudy: drops. 3. Fog: cloudy. 4. Cloudy. 5. Bright: rain. 6. Bright: cloudy. 7. Cloudy: showers. 8, 9. Bright: clear. 10. Clear. 11, 12. Cloudy: clear. 13. Clear: cloudy. 14. Bright: cloudy. 15. Clear. 16. Clear: fog. 17. Cloudy: showers. 18. Damp: fog. 19. Bright: thunder. 20. Bright: cloudy. 21. Bright: drops. 22. Cloudy: clear. 23, 24. Clear. 25. Bright: showers. 26. Clear: thunder. 27. Thunder. 28. Showers: rain. 29. Showers: cloudy. 30. Drops: cloudy. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Aug. 1. Wet all day. 2. Very wet. 3. Fair and fine. 4. Fine: one shower. 5. Fine. 6. Showers and sunshine. 7. Wet all day. 8. Wet. 9. Very clear and fine. 10. Very fine: one shower. 11. Very fine, but fair. 12, 13. Very fine. 14. Fine, but heavy rain P.M. 15. Fine, but fair. 16. Fine: fair: thunder P.M. 17, 18. Fine. 19. Fine: thunder. 20. Heavy showers A.M. 21. Fair A.M.: rain P.M. 22. Heavy rain. 23. Rain: cleared P.M. 24. Very fine. 25, 26. Rain. 27. Shower. 28. Heavy showers. 29—31. Fair and fine.

Temperature (mean) of spring-water .... 53°·5
Ditto August 1842 .... 50 ·7

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary,* Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse,* DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.							
1843. Aug.	London: Roy. Soc. 9 a.m.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Dumfries-shire. 9 a.m.	Dumfries-shire. 9 p.m.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8¾ p.m.
1.	30·022	29·934	29·849	29·38	29·68	29·65	29·62	29·61
☽ 2.	29·792	29·750	29·621	29·21	29·42	29·30	29·47	29·38
3.	29·638	29·574	29·351	29·04	29·25	29·28	29·34	29·37
4.	29·586	29·605	29·527	28·96	29·30	29·45	29·40	29·50
5.	29·848	29·858	29·774	29·22	29·60	29·58	29·54	29·57
6.	29·960	30·116	29·890	29·33	29·65	29·94	29·75	29·88
7.	30·286	30·211	30·191	29·64	29·98	29·95	30·02	29·90
8.	30·302	30·220	30·153	29·61	30·00	29·98	30·05	29·99
9.	30·136	30·060	29·950	29·46	30·01	30·00	30·00	30·13
○ 10.	30·080	30·177	29·997	29·50	30·12	30·09	30·19	30·20
11.	30·320	30·237	30·234	29·70	30·16	30·73	30·15	30·16
12.	30·322	30·237	30·213	29·70	30·18	30·18	30·13	30·28
13.	30·290	30·211	30·093	29·70	30·02	30·08	30·28	30·20
14.	30·100	30·074	29·945	29·54	29·93	29·96	30·15	30·15
15.	29·978	29·923	29·894	29·36	29·98	29·92	30·15	30·14
16.	30·052	30·040	29·979	29·43	30·04	29·99	30·15	30·09
17.	30·180	30·100	30·088	29·54	30·06	30·05	30·00	30·10
☾ 18.	30·152	30·084	29·958	29·50	29·85	29·97	30·06	30·00
19.	29·898	29·820	29·670	29·29	29·62	29·69	29·81	29·70
20.	29·726	30·171	29·647	29·00	29·83	29·79	29·62	29·68
21.	30·072	29·977	29·855	29·45	29·23	29·59	29·71	29·50
22.	29·702	29·640	29·477	29·10	29·38	29·27	29·21	29·18
23.	29·698	29·619	29·412	29·00	29·62	29·50	29·20	29·42
24.	29·692	29·787	29·599	29·08	29·65	29·70	29·65	29·50
● 25.	29·910	29·889	29·835	29·26	29·74	29·68	29·65	29·65
26.	29·986	30·068	29·929	29·34	29·95	29·82	29·72	29·73
27.	30·208	30·139	30·080	29·59	29·75	29·91	29·89	29·85
28.	30·058	29·994	29·916	29·44	29·81	29·50	29·72	29·50
29.	30·040	30·037	29·964	29·32	30·05	29·98	29·68	29·85
30.	30·180	30·138	30·092	29·59	30·09	30·07	29·93	30·05
31.	30·286	30·195	30·158	29·66	30·00	30·10	30·05	30·00
Mean.	30·015	29·996	29·891	29·38	29·804	29·826	29·815	29·814

Days of Month.	Thermometer.									
1843. Aug.	London: R.S. Fahr. 9 a.m.	London: R.S. Self-reg. Mx.	London: R.S. Self-reg. Min.	Chiswick. Max.	Chiswick. Min.	Boston. 8½ a.m.	Dumf.-shire. Max.	Dumf.-shire. Min.	Orkney, Sandwick. 9¼ a.m.	Orkney, Sandwick. 8¾ p.m.
1.	62·3	76·0	53·6	72	46	61	59	50	58	54
2.	63·7	74·2	54·6	62	52	61	58½	53½	59½	55
3.	64·0	66·7	56·5	62	54	64	64	54	59	55
4.	60·4	67·4	57·6	68	46	62	67	52	56	55
5.	62·2	75·0	54·4	72	53	62·5	63	48	57½	52
6.	62·[illegible]	66·6	56·8	70	42	63	63	48	54	54
7.	66·3	79·8	54·0	75	58	63	61½	43	60	55½
8.	69·3	72·2	61·3	80	58	68·5	62	48	58½	57
9.	69·2	77·0	63·2	82	57	67	62	52½	59	54
10.	59·0	78·8	59·3	68	44	57	68	40	58	54
11.	58·3	66·3	52·5	72	46	60·5	66	45½	58	55
12.	64·7	70·2	56·0	77	47	61	68½	47	66	52½
13.	57·8	74·0	56·5	78	60	65	71	52	59	55
14.	66·0	72·2	58·4	79	59	66	72½	51	60	55
15.	65·7	74·2	62·7	82	60	64	69	58	58½	54
16.	66·8	75·2	63·3	74	55	66·5	78	59½	61	56
17.	64·7	69·7	60·3	80	55	63	77	51	60	56
18.	68·7	75·8	63·2	84	59	65	79	55	60	56
19.	73·2	81·2	63·4	84	61	69	68½	54	64	60
20.	70·3	81·3	66·7	72	47	67	64	44	57	53
21.	59·8	74·2	52·4	71	49	56·5	62½	52	58	54
22.	62·3	68·7	56·3	62	45	61	63	46	60	50½
23.	62·2	70·3	53·4	69	53	60	66½	43	57	52
24.	56·8	71·5	55·0	73	46	58	67	51½	57	54½
25.	64·2	66·5	55·2	71	56	62·5	68½	48	58½	53½
26.	64·4	69·6	60·9	75	46	61	64	45	55½	54
27.	58·0	70·5	55·0	75	47	62	62	52	58	54
28.	64·3	70·3	56·3	71	50	63	63½	49½	58	54½
29.	65·2	68·3	60·4	74	59	66	63½	39	58½	54
30.	65·6	72·0	63·5	77	61	62	66	43	55	53½
31.	64·4	71·2	61·7	82	56	62	67	45	58½	56
Mean.	63·9	72·5	58·3	73·96	52·77	62·9	66·3	49·2	58·61	54·46

Days of Month.	Wind.					Rain.					Dew-point.
1843. Aug.	London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
1.	s.	sw.	calm	sw.	w.	......	......	·07	......	......	58
2.	s.	sw.	w.	se.	se.	......	·02	......	......	·36	59
3.	s.	sw.	w.	sw.	s.	......	1·03	......	......	·03	60
4.	s.	sw.	w.	w.	w.	·033	·18	·28	......	·03	59
5.	w.	w.	nw.	w.	sw.	·122	·02	·25	......	......	58
6.	w.	w.	w.	w.	w.	......	......	·11	......	·25	57
7.	s.	sw.	calm	sw.	se.	......	......	......	1·30	......	59
8.	s.	sw.	w.	sw.	e.	......	......	......	......	......	61
9.	sse.	s.	calm	w.	wsw.	......	......	......	......	......	63
10.	nw.	ne.	calm	sw.	w.	......	......	1·22	......	......	62
11.	n.	n.	w.	ssw.	se.	......	......	......	......	......	58
12.	nw.	n.	w.	ssw.	sw.	......	......	......	......	......	60
13.	ne.	se.	calm	ssw.	se.	......	......	......	......	......	55
14.	e.	e.	s.	sse.	se.	......	......	......	......	......	62
15.	e.	s.	nw.	e.	se.	......	·61	·18	0·20	......	64
16.	w.	sw.	calm	s.	se.	·019	·16	·41	......	......	63
17.	sw.	n.	calm	w.	w.	.055	......	·58	......	......	63
18.	n.	e.	calm	e.	...	......	......	......	......	·17	66
19.	ne.	e.	e.	s.	sse.	......	·01	......	......	......	70
20.	s.	s.	w.	w.	wsw.	......	......	......	......	......	68
21.	nw.	nw.	wnw.	s.	se.	·155	·02	·33	......	·20	58
22.	s.	s.	wsw.	sse&s.	se.	......	·22	......	......	·19	56
23.	s.	s.	w.	s.	s.	·202	·95	·11	1·70	......	58
24.	nw.	w.	calm	sw.	se.	1·408	......	·39	......	......	59
25.	s.	s.	sw.	s.	se.	......	......	......	......	......	60
26.	s.	sw.	calm	s.	se.	......	·01	......	......	·10	60
27.	s.	nw.	calm	s.	s.	......	......	......	......	·10	57
28.	s.	s.	calm	se.	se.	......	·05	......	......	·25	61
29.	s.	sw.	w.	nw.	sw.	·036	......	·06	......	·16	62
30.	sw.	sw.	calm	w.	sw.	......	......	·04	0·55	......	63
31.	e.	se.	calm	s.	s.	......	......	......	......	......	62
Mean.						Sum. 2·030	3·28	4·02	3·75	1·84	Mean. 60

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XXXVIII. *On the Results of the Panary Fermentation, and on the Nutritive Values of the Bread and Flour of different countries.* By Robert D. Thomson, *M.D., Conductor of the Laboratory and of the Classes of Practical Chemistry in the University of Glasgow*.*

SEVERAL years have elapsed since the author first had his attention directed to the comparative chemical and medical values of fermented and unfermented bread as articles of food. The common idea, which yielded the palm of superiority to the former, did not appear to be based on solid data, and it was therefore considered desirable that, in reference to a subject of such importance to the nourishment of man, the arguments in favour of such an opinion should be subjected to a careful examination. Judging *à priori* it did not seem evident that flour should become more wholesome by the destruction of one of its important elements, or that the vesicular condition of bread could alone be gained by a process of fermentation.

When a piece of dough is taken in the hand, being adhesive and closely pressed together, it feels heavy, and if swallowed in the raw state, it would prove indigestible to the majority of individuals. This would occur from its compact nature and from the absence of that disintegration of its particles, which is the primary step in digestion. But if the same dough were subjected for a sufficient length of time to the elevated temperature of a baker's oven (450°), its relation to the digestive powers of the stomach would be changed; because the water to which it owed its tenacity would be expelled, and the only obstacle to its complete division and consequent subser-

* Abstract of papers read before the Philosophical Society of Glasgow, 14th February 1842, and 26th April 1843; and now communicated by the Author.

viency to the solvent powers of the animal system would be removed. This view of the case is fully borne out by a reference to the form in which the flour of the various species of *Cerealia* is employed as an article of food by different nations. By the peasantry of Scotland, barley-bread, oat-cakes, peas-bread, or a mixture of peas- and barley-bread, and also potatoe-bread mixed with flour, are all very generally employed in an unfermented form, with an effect the reverse of injurious to health. With such an experience under our daily observation it is almost superfluous to remark, that the Jew does not labour under indigestion when he has substituted, during his passover, unleavened cakes for his usual fermented bread, —that biscuits are even employed when fermented bread is not considered sufficiently digestible for the sick, and that the inhabitants of the northern parts of India and of Affghanistan very generally use unfermented cakes, similar to the *scones* of Scotland.

Such then being sufficient evidence in favour of the wholesomeness of unfermented bread, it becomes important to discover in what respect it differs from fermented bread. Bread-making being a chemical process, it is from chemistry alone that we can expect a solution of this question. In the production of fermented bread, a certain quantity of flour, water and yeast are mixed together and formed into a dough or paste, which is allowed to ferment for a certain time at the expense of the sugar of the flour. The mass is then exposed in an oven to an elevated temperature, which puts a period to the fermentation, expands the carbonic acid resulting from the decomposed sugar and the air contained in the bread, and expels the alcohol formed and all the water capable of being removed by the heat employed. The result gained by this process the author considers to be merely the expansion of the particles of which the loaf is composed, so as to render the mass more readily divisible by the preparatory digestive organs. But as this object is gained at a sacrifice of the integrity of the flour, it becomes a matter of interest to ascertain the amount of loss sustained in the process. To determine this point the author had comparative experiments made upon a large scale with fermented and unfermented bread. The latter was raised by means of carbonic acid generated by chemical means in the dough; but to understand the circumstances some preliminary explanation is necessary.

Mr. Henry of Manchester, at the end of the last century, suggested the idea of mixing dough with carbonate of soda and muriatic acid, so as to disengage carbonic acid in imitation of the usual effect of fermentation; but with this advantage, that

the integrity of the flour was preserved, and that the elements of the common salt required as a seasoner of the bread, were thus introduced and the salt formed in the dough. Dr. Hugh Colquhoun first, it is believed, carried this suggestion into practice, in 1826, and made numerous experiments on bread-making*. But it was not till within a very few years that the idea of using bread thus baked on a large scale was carried into execution. From the result of several experiments made at the author's request, it appears that upon an average there is a great loss sustained by flour when it is fermented. In comparison with the bread raised by carbonate of soda and muriatic acid, there is a loss in the sack of flour of 30lbs. 13oz.; or in round numbers a sack of flour would produce 107 loaves of unfermented bread, and only 100 of fermented bread of the same weight. Hence it appears that, by the common process of fermented baking, in the sack of flour, 7 loaves, or $6\frac{1}{2}$ per cent. of the flour, are driven into the air and lost†. An important question now arises from the consideration of the result of this experiment, viz. does the loss arise entirely from the decomposition of sugar, or is any other element of the flour attacked?

It appears from a mean of 8 analyses of wheat flour from different parts of Europe by Vauquelin, that the quantity of sugar contained in flour amounts to 5·61 per cent. But it is obvious that as the quantity lost by baking exceeded this amount by nearly 1 per cent., the loss cannot be accounted for by the removal merely of the ready-formed sugar of the flour. We must either ascribe this extra loss to a conversion of a portion of the gum of the flour into sugar and its decomposition by means of the ferment, or we must attribute it to the action of the yeast upon another element of the flour; and if we admit that yeast is generated during the panary fermentation, then the conclusion would be inevitable that another element of the flour, besides the sugar or gum, has been affected. For Liebig has well illustrated the fact, that when yeast is added to wort, ferment is formed at the expense of the gluten, while the sugar is decomposed into alcohol and carbonic acid. Now in the panary fermentation, which is precisely similar to the fermentation of wort, we might naturally expect that the gluten of the flour would be attacked to reproduce yeast.

* Annals of Philosophy, N.S., vol. xii.

† In consequence of these and other facts brought forward by the author, the unfermented system of baking has been introduced into many of the unions in England, where he believes it has been found that he has not overrated the saving, which the above experiments would indicate to be upwards of a fifteenth.

The author has succeeded in forming a wholesome and palatable bread by the employment of ammoniacal alum and carbonate of ammonia or soda as a substitute for yeast. In this process the alum is destroyed by the heat; the bread is vesicular and white, and rises, according to the judgement of the baker, as well as fermented bread. It is obvious that none of the ingredients added can affect the integrity of the constituents of the flour, an occurrence which possibly may happen in the preparation of bread by the common process of fermentation, as has been shown, even to the azotized constituents. The disadvantage of such a deterioration is sufficiently evident if we view these principles as the source of nutrition in flour.

The first chemist who examined flour with any successful result was Beccaria of Bologna, who detailed his experiments in a communication to the Academy of that place, in 1742. "To endeavour to know oneself," observes he, "is to satisfy the obligation which the oracle of Apollo imposes on every one—*to know oneself*—for, if we except the spiritual and immortal part of our being, and if we only take into consideration our bodies, is it not true that we are composed of the same substances which serve as our nourishment?*" From his subsequent remarks it is obvious that he considered the glutinous part of flour to be peculiarly of an animal and the starch of a vegetable nature; for when distilled, the gluten, he says, affords principles similar to those of all animals, while the starchy part yields products similar to those of all vegetables. We have thus, in the sagacious observations of Beccaria, the origin of the present idea, that animals are principally formed from the glutinous or albuminous principle of vegetables. The mechanical method of analysis which the Italian chemist discovered is the basis of our present process, and it affords undoubtedly the only test which we possess of the comparative value of flour as a baking material by the fermented plan. But it fails to inform us of the absolute nutritive value of flour. The most correct method of accomplishing this object is by the determination of the amount of azote present in the flour, by converting that element into ammonia, and precipitating by bichloride of platinum. In the following analyses, to determine the comparative values of different kinds of bread and flour, this process has been used, and the nutritive principles calculated by considering them to contain on an average 16 per cent. of azote, according to Dumas.

I. Naumburg. Bread with a brown aspect. This town is situated in the south of Prussia, on the river Saale, in the neighbourhood of a fertile country. The specimen was ob-

* *Collection Académique*, vol. x. p. 1.

tained by the author at the Preussischen Hof, on the 17th August 1842, and as harvest was only commencing, the flour of which it was baked would be in all probability of the growth of 1841. The same observation applies to all the German specimens:—10 grs. pulverized and dried at 212° Fahr., being heated with a mixture of lime and soda, yielded, after precipitation of the ammonia formed by bichloride of platinum, washing and ignition, 1·80 grs. platinum = ·2639 gr. azote.

II. Dresden. White bread from the Stadt Rom, procured 21st August 1842, probably therefore of the growth of 1841:—10 grs. afforded 1·57 gr. platinum = ·2289 gr. azote.

III. Berlin white bread, procured 22nd August 1842, in the Stadt Rom:—10 grs. gave 1·56 gr. platinum = ·2275 gr. azote.

IV. Canada flour, probably of the growth of 1842. The same observation applies to the subsequent specimens:—9·9 grs. gave 1·5 gr. platinum = ·221 gr. azote.

V. Essex flour:—9·1 grs. gave 1·3 gr. platinum = ·2175 gr. azote.

VI. Glasgow unfermented bread, raised by means of muriatic acid and soda:—10 grs. afforded 1·47 gr. platinum = ·21437 gr. azote.

VII. Lothian flour:—10 grs. gave 1·35 gr. platinum = ·1968 gr. azote.

VIII. United States' flour:—10 grs. gave 1·25 gr. platinum = ·182 gr. azote.

This experiment appeared to place the United States' flour very low in the scale. The flour was therefore analysed by the mechanical method, and the following result obtained. The quantity used was 3 ounces.

				per cent.
Starch			902·00	68·73
Gluten	Fibrin	116·80	130·40	9·93
	Casein	5·27		
	Glutina oil	3·04		
	Loss (water)	5·29		
Albumen			14·00	1·06
Gum			60·40	4·60
Sugar			16·30	1·24
Water			189·40	14·44
			3 oz. = 1312·50 grs.	100·00

By the first experiment the platinum obtained indicated the presence of 11·37 per cent. of azotized principles, and by the mechanical method the amount was 10·99, a very close approximation. In the latter analysis all the products were dried at 212° until they ceased to lose weight.

In the following table the results of the preceding analyses are collected, so as to exhibit the comparative value of each specimen. The first column gives the amount of azotized principles contained in each, and the secoud column represents their equivalent values in the nutritive scale.

	Azotized principles. per cent.	Equivalents.
1. Naumburg bread	16·49	100·00
2. Dresden bread	14·30	115·31
3. Berlin bread	14·21	116·04
4. Canada flour	13·81	117·23
5. Essex flour	13·59	121·33
6. Glasgow unfermented bread	13·39	123·15
7. Lothian flour	12·30	134·06
8. United States' flour	11·37	145·03
Ditto, by mechanical analysis	10·99	150·00

This table shows that the German and Canada flour contain most nutritive matter; the Essex flour being only a slight degree lower in the scale. It must be borne in mind, however, that this result may not be in consonance with the opinion of the baker in reference to the capacity of the flour for making good bread, because it takes in another element, the albumen, which is omitted in the baker's estimate. It is therefore quite possible that the specimen holding the lowest position in the table may answer the purpose of the baker in an equal or superior manner to those placed above it; but the method of determining the comparative value of flour by the estimation of the azote may furnish us at once with data of utility both in commerce and œconomy*.

XXXIX. *On the Production of Heat by the Contraction of Elastic Tissue, in reference to a former communication.* By J. M. Winn, *M.D.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN Dr. Gregory's translation of Liebig's 'Animal Chemistry,' I find at page 31 the following remark:—"The observation has been made that heat is produced by the contraction of muscles, just as in a piece of caoutchouc, which, when rapidly drawn out, forcibly contracts again with disengagement of heat." With the exception of an essay in the Lancet

* The result of Sir H. Davy in reference to the quantity of gluten in British flour, is sometimes nearly the double of the numbers in the table. This may perhaps be ascribed to his mode of drying the gluten.

for September 2, 1843, in which the writer attempts to appropriate my views, I do not know of any observations to which the Professor can allude, but those which I published in your Journal for March 1839 [S. 3. vol. xiv. p. 174], and if he will refer to them he will perceive that he has not quite understood my notions. My experiments were made with elastic and not muscular tissue, and the increase of heat in the caoutchouc operated on was observed immediately after it had been elongated and before it had been allowed to re-contract.

As the Professor's imperfect explanation of my views might bring them into some discredit, I shall feel obliged by your publishing this Note.

I have the honour to be, Gentlemen,

Your obedient Servant,

J. M. Winn.

Truro, Sept. 21, 1843.

XL. *The Leaf-stalks of Garden Rhubarb as a Source of Malic Acid.* *By* Thomas Everitt, *Esq.**

THE large quantity of this substance which is brought to our vegetable markets for several months in the year, beginning very early in spring, and its powerful though agreeable acid taste, make it a subject worthy of a more minute chemical examination than any which it has as yet been subjected to.

The leaf-stalks of garden rhubarb were first examined by Mr. Henderson†, who discovered in them, as he thought, a peculiar acid; afterwards by M. Lassaigne‡, who showed that the supposed new acid was oxalic acid. But these experimenters examined only the precipitate obtained by putting chalk into the expressed juice; the first-named decomposing the insoluble precipitate thus obtained by sulphuric acid; the other, by boiling it with excess of carbonate of potassa, then neutralizing the solution with nitric acid, and precipitating by a salt of lead, decomposing the latter by sulphuretted hydrogen, and thus getting crystals which were oxalic acid. Now by both these processes, those chemists threw away, in the liquid which floated above the oxalate of lime, an important constituent in a large quantity, viz. malate of lime, with a great many other things of less importance, but which rendered the

* Communicated by the Chemical Society; having been read February 7, 1843.

† Thomson's Annals of Philosophy, vol. viii. p. 247. (1816.)

‡ *Annales de Chimie et de Physique*, tom. viii. p. 402. (1818.)

devising of a process for obtaining the principal ingredients pure, a difficult analytical problem. The details of the preliminary experiments (which occupied some time) for finding out what I had to deal with, would be both tedious and useless; I proceed, therefore, to give a summary of the method I adopted for the analysis of this substance, and of the best means of proceeding, if the extraction of malic acid be the only object. The stalks should have the cuticle taken off, as it would introduce a great deal of colouring matter if put into the press; the peeled stalks are cut into small pieces about an inch long, put into a strong canvas bag, and then subjected to a great pressure; by this means 20,000 grains of peeled stalks yielded 12,500 grains of juice, and left 3850 grains of damp fibre, which well washed and dried at 212°, weighed 800 grains, and is equal to 4 per cent. ligneous fibre. The liquid had a light green colour, was very acid, its density varied with the size of the stalks and the time elapsed since they were cut, it also varied in the same specimen at different periods of the pressing; that which flows first I have had as low as 1·015, rising to that last yielded 1·022.

I tried how much pure carbonate of soda and carbonate of potassa were required to saturate a definite quantity; but as it afterwards was found to contain two or three acids, and some salts of soda and potassa also present, these results are of no use for determining the quantity of free acid. Some pure crystals of carbonate of lime were made into a neutral nitrate; chloride of calcium being avoided as in a subsequent stage chloride of lead, and hydrochloric acid would be formed, to get rid of which would have complicated the process.

To several pints of the juice bicarbonate of potassa was added, this salt being used because it is much purer than the carbonate, until all acidity was neutralized: a small quantity of greenish pulpy matter made its appearance, which was separated by a cloth filter, and the liquid became much less coloured: 4000 grains measure, specific gravity 1·012, required 65 grains of crystallized bicarbonate potassa for neutralization: 4000 at 1·023 required 93 of the same for neutralization: nitrate of lime was now added and the solution boiled: this is necessary, because oxalate of lime, when precipitated cold and thrown on a paper filter without boiling, passes through; moreover, malate of lime requires only 65 parts of boiling water to hold it in solution. I found the separation of the oxalic acid perfect by these means, while all the malates remained in solution. The oxalate of lime collected on the filter, amounted to 24·2 grains, dried at 212°, or the protohydrate. It was tested in the usual way of boiling with ex-

cess of carbonate of soda or potassa, filtered, neutralized with nitric acid, and precipitated by nitrate of silver; the powder dried and heated exploded feebly in the manner peculiar to the oxalate of silver, leaving metallic silver. Nitrate of lead was now added to the solution which had passed through the filter from the oxalate of lime, and a copious bulky precipitate was formed, which the next day, when it was cold, had formed on its surface a few of the beautiful flat pearly crystals characteristic of malate of lead: it was brought to the boil, and the malate of lead assumed a consistency like dough before it goes into the oven; and when that cooled it became as brittle as resin. The liquid above the solid mass was decanted and yielded good crystals on cooling. The whole malate of lead was carefully washed and elutriated. About two-thirds of it was acted on afterwards by sulphuric acid gently heated; then separating the sulphate of lead by a filter, the remaining third of malate lead was suspended in the liquid, and sulphuretted hydrogen passed through it till all the malic acid was set free. This decomposition of some of it by sulphuretted hydrogen, renders the solution sufficiently colourless, while to do the whole in this way is very tedious. When operating on several ounces, after filtration to collect the sulphuret of lead, the free malic acid must be evaporated by a water or steam bath to the thickness of syrup; and it was only obtained of the consistency of thick honey, by keeping it under the receiver of an air-pump, near the surface of oil of vitriol, for nearly a week, occasionally taking out the capsule to warm it. Some of the original juice, evaporated in the same way, yielded beautiful crystals of binoxalate of potassa.

After this the liquor still retained a small trace of citric acid, which I obtained in a distinct form by taking advantage of the difference of the solubility of malate and citrate of baryta. No tartaric could be detected.

4000 grains of the original liquid evaporated and ignited in platinum, yielded 29·2 grains of ashes, of which

28·4 grains were soluble in water, and 0·8 insoluble.

The solution of these 28·4 grains was alkaline, and required 12 real nitric, or 8·9 of sulphuric acid to neutralize it (these two acids were used of an exact strength so as to contain 1 grain of real acid in 100 grains water measure). To the neutral solution of the ashes in sulphuric acid nitrate of baryta was added; a precipitate was formed, part of which was soluble on adding a little nitric acid, and turned out to be phosphate. The insoluble sulphate weighed 39·73 grains: no lime salt was present. The nitric acid solution of the phosphate evaporated to dryness left 4·4 grains; it was well tested and

certainly proved to be phosphate, being made into an alkaline phosphate and tested by silver and other means. To the solution which filtered from the mixed sulphate and phosphate, containing excess of nitric acid, nitrate silver was added and 4·1 of chloride obtained; to the solution filtered from the chloride of silver, excess of hydrochloric and sulphuric acids were added to remove the excess of silver and baryta: the filtered liquid evaporated to dryness and ignited, after putting on it excess of sulphuric acid, gave 39·3 grains of a white salt, quite soluble in water (sulphates of soda and potassa); these were dissolved in a minimum of water, and excess of crystals of tartaric were added; the granular precipitate washed with dilute alcohol, weighed 33·3; the solution from the bitartrate of potassa evaporated and ignited, then treated with sulphuric acid, evaporated again to dryness and ignited, gave of dry sulphate of soda 4·7; this was dissolved in water, and being slowly evaporated, the characteristic crystals were formed.

From the above data, and from some subsequent experiments on a much larger quantity of juice, an imperial gallon (sp. gravity 1·022), contains nearly, of

Malic acid dry	11139·2 grains.
Oxalic acid dry	320·6 „
Potassa combined with organic-chloride, soda, sulphuric and phosphoric acids, traces of silicon and a little vegetable extract	229·6 „

If to obtain malic acid be the only object, slaked lime made into a sort of cream with water might be added to the expressed juice, till the solution became slightly alkaline; it might then all be boiled and filtered, then proceed with the nitrate of lead and the rest of the steps above described. To procure the malate of lead in good crystals, some precautions are necessary. From the precipitate suspended in water and heated, a few grains only fall on cooling; from 2 pints I only obtained $5\frac{1}{2}$ grains; but if about 2 per cent. of acetic, or of some free malic, be added to the water, and finely divided malate of lead be added, and the whole warmed by a water bath, with constant stirring, the quantity of crystals will be doubled for the same bulk of liquid. It is proper not to raise the temperature higher than 160° Fahr.; if boiled the salt loses two or three atoms of its water of crystallization, and then is quite insoluble in water hot or cold. The composition of malic acid is exactly the same as that of citric acid, $C_4\,H_2\,O_4$.

The crystals of malate of lead are thus constituted:—

1 proportion of acid . . .	58	...	29·68
1 proportion of oxide of lead	112	...	56·62
3 proportions of water . .	27	...	13·70
			100·00

100 grains of the crystals exposed in a thin stratum on a porcelain dish, can lose at 212° 9·2 water = 2 proportions, but it required to be heated in a thin glass tube, by means of an oil-bath to 356° Fahr. before it lost the other third. When the crystals are boiled in water, they lose also 2 proportions of water, and assume the form of dough after it has been kneaded; the mass on cooling becomes as brittle as resin.

XLI. *Examination of Astringent Substances* (continued). *By* John Stenhouse, *Esq., Ph.D.**

Black and Green Tea. GREEN and black tea are said by Mulder, the chemist who has most recently examined the subject, to be both derived from plants of the same species. The differences observable in them are, as he alleges, chiefly owing to their being collected at different periods of their growth, and to the greater or less degree of heat with which they are subsequently dried; the black teas being strongly heated upon iron plates, while the green teas are exposed to a comparatively moderate temperature. If this statement is correct, it may serve to explain what has been long observed, that an aqueous infusion of black tea, though quite transparent while hot, becomes muddy on cooling, while an infusion of green tea retains its transparency even when quite cold. The reason of this difference probably is, that most of the essential oil of black tea is converted, by the partial roasting it has undergone, into a resinous matter, which though soluble in hot is nearly insoluble in cold water, while the essential oil of green tea, on the contrary, remains nearly unchanged, which is probably the cause both of the clearness of its solution and perhaps also of the more powerful effect which green tea is well known to exert on the animal œconomy.

The aqueous infusion of both green and black tea give dull olive-black precipitates with protosulphate of iron, which on standing become leaden black. Infusions of tea also, when evaporated to dryness and distilled, give crystals of theine

* Communicated by the Chemical Society; having been read Feb. 21, 1843. The former part of Dr. Stenhouse's paper appeared in Phil. Mag., S. 3. vol. xxii. p. 417.

which collect on the sides and neck of the retort, while the empyreumatic liquor which passes into the receiver gives pretty distinct indications of containing pyrogallic acid.

The Tannin of Tea.—In order to separate the tannin of tea from the other proximate principles of the plant, its aqueous infusion was precipitated with acetate of lead, and the precipitate carefully washed with hot water. Green tea gave a bright yellow precipitate, but that of black tea had a brownish-yellow colour. The lead salts were decomposed by sulphuretted hydrogen: the solution of the tannin of green tea had only a slight yellow colour, while that of black tea had a much darker colour, but in other respects the properties of both appeared to be the same. The following are the effects upon them of different reagents:—with solution of gelatine they gave white bulky precipitates, and they also gave copious white precipitates with tartar-emetic. Protosulphate of iron throws down bright bluish-black precipitates, nitrate and chloride of iron, olive black, and acetate of iron purple black precipitates. The solution of the tannin when evaporated to dryness on the water-bath, became of a reddish-brown colour, and was partially decomposed.

When this tannin was subjected to distillation, it invariably yielded a quantity of pyrogallic acid, which sometimes appeared in crystals upon the sides of the retort, but which more frequently remained dissolved in the empyreumatic liquor which passed into the receiver. In this it was easily detected by the usual reagents. It gave a fine reddish-purple colour when dropped on milk of lime, with protosulphate and protonitrate of iron, a fine indigo-blue colour, and with protochloride, a blue resembling ammoniuret of copper. As the quantity of pyrogallic acid obtained was always much less than that which the same quantity of the tannin of either galls or shumac would have yielded, I was led to suspect that it did not arise from the decomposition of the tannin in the tea, but resulted from some gallic acid with which the tannin was mixed. Of the accuracy of this opinion I was speedily convinced by the following experiment:—On treating a strong solution of the tannin with nearly half its bulk of sulphuric acid added by little and little at a time, a dark brown precipitate fell consisting of the tannin combined with the acid. It was however much more soluble than the corresponding compound of the tannin of galls. It was collected on a cloth filter, strongly compressed, and washed with a little cold water to free it as much as possible from adhering acid. When subjected to distillation it did not afford the slightest trace of pyrogallic acid, showing evidently that the pyrogallic acid

I had previously obtained was not derived from the tannin of the tea. When another portion of the precipitated tannin was boiled with tolerably dilute sulphuric acid, it did not yield any gallic acid, but was changed into a dark brown substance, nearly insoluble in cold, and but very little more so in boiling water. It gave a grayish black precipitate with protosulphate of iron, but was not precipitated either by gelatine or tartar-emetic. It dissolved however pretty easily both in alcohol and alkalies, forming dark brown solutions. It is evident therefore that though in some of its properties the tannin of tea agrees pretty closely with that of nut-galls, still the products of its decomposition are essentially different.

The tea was next examined for the gallic acid which it evidently contained, and this I was always able to procure by either of the following methods:—The mixture of tannate and gallate of lead obtained by precipitating a decoction of tea by acetate of lead, was decomposed as before by sulphuretted hydrogen and evaporated to dryness. It was then macerated with a very little cold water which removed most of the tannin, but dissolved scarcely any of the gallic acid. The residue was again dried, reduced to powder and mixed with some sand, was repeatedly agitated with æther in a stoppered bottle. The æthereal solution was then poured off, and almost the whole of the æther was recovered by distillation. The residue when left to spontaneous evaporation deposited crystals, which at first had a yellow colour, but which were rendered perfectly white by a second crystallization. The other process was somewhat more tedious, but by it very small quantities indeed of gallic acid can be detected. It consists in putting a number of bits of prepared skin into the mixed solution of tannin and gallic acid already mentioned, and allowing them to remain for nearly a fortnight till the whole of the tannin is absorbed by the skin. The gallic acid is then precipitated by acetate of lead, and the precipitate having been well washed, first with hot water and then with spirits of wine, is to be decomposed by sulphuretted hydrogen.

When evaporated to dryness and treated with æther as before, crystals of gallic acid are readily obtained, which are at first much purer than those got by the former method. I examined several specimens both of black and green tea, and also one of Assam tea, in every instance with similar results. It is evident, therefore, that tea, besides a species of tannin which gives bluish-black precipitates with protosulphate of iron, invariably contains a small but constant quantity of gallic acid, a constituent which has hitherto been overlooked.

Myrobalans.—The name Myrobalans is applied to the fruit of several East Indian trees, the species of which are, I believe, not yet all accurately determined. That which I examined was the yellow kind, the fruit of the *Jerminalia Chebula*. The ripe fruit has a brownish-yellow colour, is pear-shaped, and deeply wrinkled. It consists of a white pentangular nut containing a small white oily kernel, and is covered by a mucilaginous and very astringent husk, nearly two lines in thickness. Each of the fruit weighs from 70 to 100 grains, and of this 50 or 60 grains are husk. It is in the husk that the whole of the astringent matter is contained, and it may be easily separated from the nut by slightly pounding or bruising the fruit. The powder of the husk is dark yellow, and its taste is very sharp and astringent. The colour of its aqueous infusion is deep yellow. With protosulphate of iron it gives a deep bluish-black precipitate, which is rather deficient in lustre. The dullness of the colour is owing to the presence of impurities in the husk, for on purifying the astringent matter by precipitating it with acetate of lead, and then decomposing the lead compound with sulphuretted hydrogen, the solution thus obtained gives as fine a colour as can be procured from infusion of galls. With gelatine it gives a very copious, slightly yellow precipitate, the quantity of astringent matter contained in myrobalans being very considerable. With tartar-emetic it also gives a copious brownish-yellow precipitate. With protonitrate and protochloride of iron, it gave bluish-black precipitates, which soon changed to olive-black, and with acetate of iron, a fine purple-black precipitate. When the decoction of myrobalans is evaporated to dryness and distilled, it yields abundance of pyrogallic acid; this I found, however, to be derived, not from the decomposition of the tannin it contains, but from a quantity of ready-formed gallic acid. Sulphuric acid occasions a very scanty dark brown precipitate in the infusions of myrobalans, if at all dilute, as the combination which this tannin forms with sulphuric acid is pretty soluble. From concentrated solutions, the tannin is readily precipitated as a yellowish-brown tenacious mass. Having been collected on a cloth filter, and freed as much as possible from adhering acid, it was dried and distilled. It yielded no pyrogallic acid, and scarcely any empyreumatic oil; another portion of the same tannin, though boiled in dilute sulphuric acid, was not converted into gallic acid, but changed into a dark insoluble mass.

Gallic acid may be readily obtained from myrobalans by precipitating its decoction with a solution of glue, filtering and evaporating to dryness. On treating the residue with

æther, pouring off the solution, recovering the greater portion of the æther by distillation, and leaving the remainder to spontaneous evaporation, crystals of gallic acid were deposited in a few hours. The quantity of gallic acid in myrobalans is pretty considerable.

Besides tannin and gallic acid, myrobalans contains a good deal of mucilage, and a brownish-yellow colouring matter, which Dr. Bancroft states was employed in India in his time as a yellow dye. Myrobalans have long been employed by the calico-printers of India instead of galls, and from the large quantity of astringent matter they contain, I think perhaps they might be worth the attention of the tanners and calico-printers of this country. A decoction of myrobalans makes a very tolerable ink, which however, as we have already stated, is rather deficient in lustre.

Bistort, Polygonum Bistortus.—The root of this plant, which is pretty common in Scotland, has a pale pink colour internally, but when it is exposed to the air for some time it becomes deep yellow. Its aqueous solution is yellowish at first, but on standing it assumes a fine red colour, and the same effect is immediately produced by boiling it with any of the alkalies. With protosulphate of iron it gives a bluish-black precipitate, a good deal resembling that of galls, but having a bluish-purple shade. Gelatine produces a copious brownish precipitate in a solution of bistort, which shows that the quantity of astringent matter it contains is considerable. With tartar-emetic it gives a brownish-white precipitate. When extract of bistort is evaporated to dryness and distilled, it gives distinct indications of pyrogallic acid. The pyrogallic acid however, as in the case of myrobalans, was derived not from the tannin in bistort, but from a quantity of gallic acid with which it was mixed, for on precipitating the tannin by sulphuric acid, and distilling it alone, not a trace of pyrogallic acid was obtained, and when boiled with sulphuric acid it was not converted into gallic acid.

The gallic acid it contains was easily obtained from bistort by precisely the same process as that already described. Its quantity, compared with that of the tannin in the root, was very considerable.

Besides tannin and gallic acid, bistort contains a brownish-red colouring matter, and a quantity of mucilage. Bistort may likewise be made to furnish a very tolerable ink, which appears to stand very well. It has a bluish-purple shade, owing to the reddish colouring matter of the root.

The Cashew Nut.—The outer rind of the Cashew nut, the fruit of the *Anacardium longifolium*, contains a considerable

quantity of a species of tannin which gives bluish-black precipitates with the sulphate, nitrate and chloride of iron, and a bluish-purple precipitate with the acetate. It is also readily precipitated by gelatine, but not by tartar-emetic. This tannin is mixed with a small quantity of gallic acid. The shell of the fruit also contains a good deal of a fatty matter, which is solid at ordinary temperatures and crystallizable. It is easily saponified when boiled with an alkali, its compound with soda crystallizes in large scales. This fat contains an acrid substance which vesicates, but it contains no sulphur. When the fat is first expressed from the nut it is but slightly coloured, but by exposure to the air it becomes first brown and then black, and loses much of its acrimony.

Pomegranate Rind.—The rind of the pomegranate contains a considerable quantity of a species of tannin which precipitates gelatine copiously, but gives only a very feeble precipitate with tartar-emetic; with protosulphate, chloride and nitrate of iron, it gives precipitates which are at first deep blue but almost immediately change to very dark olive. With acetate of iron it gives a purple precipitate. Reuss, who has made an analysis of pomegranate rind, states that he found it to contain a little gallic acid. I have been unable to find any, though I have sought it very carefully.

Larch Bark.—The bark of the larch is employed in Scotland to some extent in tanning. The quantity of tannin it contains is considerable, but the leather made with it is of inferior quality. The aqueous solution of the bark is strongly acid to test paper, and has at first a pale yellow colour, which exposure to the air renders brownish-red; it gives a copious fawn-coloured precipitate with gelatine, but none with tartar-emetic. With the sulphate, chloride and nitrate of iron, it gives olive-green precipitates. Acetate of iron throws it down of a bluish-purple colour. Sulphuric acid precipitates it of a reddish-yellow colour. When boiled with the acid it dissolves, and the liquid assumes a fine scarlet colour like the infusion of Brazil wood. The altered tannin precipitates on cooling in beautiful red flocks, as it is but little soluble in cold water. It is very soluble in alcohol and alkalies, and its solutions have a rich scarlet colour, which is the most characteristic reaction of this species of tannin. Larch bark also contains a good deal of mucilage and resinous matter. Birch bark, alder bark, and tormentil root, contain all of them considerable quantities of tannin, which closely resemble that of larch bark. All these species of tannin are readily precipitated by gelatine, but not by tartar-emetic. They give olive-green precipitates with most of the salts of iron except the

acetate, which throws them down of a bluish-purple colour, which on standing changes to a leaden-gray. When boiled with alkalies they immediately assume a fine red colour, but they differ from the tannin of the larch in not being reddened by sulphuric acid. I think it unnecessary to go into more minute details respecting them, as I have been unable to derive from them any determinate or crystalline compounds. I shall leave this subject, therefore, for the present with one or two general observations.

The great difficulty of examining the different species of tannin with a view to classifying them, is chiefly owing to their amorphous nature, to the great similarity of their properties, and to the circumstance, that except in the case of nut-galls and shumac, the products of their decomposition are of a very indeterminate character. We think however that there are good grounds for believing that both nut-galls and shumac contain the same species of tannin, for the effects of reagents upon it are exactly the same, and the products of its decomposition, when boiled with either sulphuric or muriatic acid, when destructively distilled, or when left to spontaneous decomposition, are in every instance identical, from whichever of these sources it has been derived. It is remarkable also that in so many instances, in eight cases out of ten which I have examined, the species of tannin which give bluish-black precipitates with protosulphate of iron are accompanied with larger or smaller quantities of gallic acid. In the present state of our knowledge it is impossible to say whether the gallic acid has originally existed in these substances, or has resulted from the decomposition of the tannin they contain. In the case of galls and shumac the latter opinion is probable enough, as we are easily able to effect this change by artificial means, and it also, as is well known, occurs spontaneously. In the case of the other species of tannin, however, we are still unacquainted with any instance of a similar transformation. It is to be hoped that subsequent researches may yet throw light on this very obscure subject. It is also rather singular that in the case of some of those species of tannin which give green precipitates with salts of iron, a somewhat similar circumstance occurs. Thus the tannin of catechu is accompanied by a crystalline acid body, catechine, which also gives green precipitates with salts of iron. I have likewise observed that in the case of infusions of birch bark, alder bark, &c., when the whole of the tannin they contain had been removed by gelatine, the clear liquid when filtered still contained a substance which precipitated salts of iron olive-green, just as the tannin had done, and which threw down salts of lead as copious dark yel-

low precipitates. When the lead salts were decomposed by sulphuretted hydrogen, I obtained an amorphous acid substance of a bright yellow colour, which was soluble in water, alcohol and æther, but which did not appear to be crystallizable.

XLII. *On the application of a new Method to the Geometry of Curves and Curve Surfaces.* *By* J. W. Stubbs, *B.A.*, *Trinity College, Dublin.*

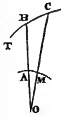
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAD the honour of reading a paper before the Philosophical Society of Dublin, on a new Geometrical principle, which as far as I am aware has hitherto escaped the notice of mathematicians. May I ask of you the favour of inserting it in your valuable Journal?

The principle consists in taking the inverse of curves and surfaces, by means of which we readily find conjugate properties to those possessed by every known curve and surface, the discussion of many of which would be impossible by the ordinary methods. If in the plane of a curve we take any point as a pole and produce the radius vector, so that the rectangle under radius vector to the original curve and the whole produced radius be constant or equal to k^2, we may call the locus of the extremity of this produced line the inverse curve to the one from which it is produced, and the extremity of the produced radius the inverse point to the extremity of the original: as an example, the cardioide is the inverse of the parabola, the focus being the pole; the lemniscata in the inverse of the equilateral hyperbola. The inverse of a right line is a circle, except when the pole is on the right line, when it is a right line. The inverse of a circle is a circle wherever the pole is situated, except it be on the circumference, when it becomes a line perpendicular to the diameter through the pole.

Fig. 1.



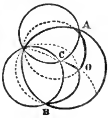
To draw a tangent to the inverse curve at the inverse point to a given point on the direct or generating curve, join the points, and on the joining line describe an isosceles triangle, one of whose sides is the tangent to the direct curve. The other will be the tangent to the inverse, as is seen by taking two consecutive radii; from the property by which it is generated the quadrilateral A M B C is circumscribable by a circle; hence the angle A M C equals the angle T B A, but in the limit the lines A M and B C become tangents: this is also clear

from this, that $r\,r' = k^2$, then $r\,d\,r' = -\,r'\,d\,r$, and $\frac{r}{d\,r} = -\frac{r'}{d\,r'}$, or $\frac{r\,d\,\theta}{d\,r} = -\frac{r'\,d\,\theta}{d\,r'}$. Hence the perpendiculars from the pole on the tangents are as the radii, and the first perpendicular being known, the second is so also.

Having established these principles, I shall proceed to show the application of this method, first to the right line and circle, afterwards to curves of the second degree, and finally to surfaces.

If a circle passing through the pole be called a polar circle; from the known theorem of the bisectors of the angles of a triangle meeting in a point, by taking the inverse of all these lines we come to the following theorem: if three polar circles form by their intersection a polar triangle A B C, the polar circles A O, B O, C O bisecting the angles meet in a point O, which is the inverse of the point in which the original bisectors meet.

Fig. 2.



From the theorem of the three perpendiculars from the angles of a triangle on the opposite sides meeting in a point, we get by inversion the three polar circles perpendicular to the opposite sides of the polar triangle, and passing through the angles meet in a point.

In like manner every theorem in plane geometry, comprising only the right line and circle, gives a conjugate one, in which right lines and circles only are contained,—every theorem, I mean, which has relation only to *position*, without introducing *lengths of lines*. I shall not mention any more of them, as, when the principle is clearly seen, that to a line corresponds a circle, and to a point a point, to the contact of a line and circle, the contact of two circles or of a line and circle according as the pole is or is not on the circumference of the circle, and to the angle between two lines, the angle between the tangents to two circles at their point of intersection, any one can multiply theorems at will.

The general equation of a conic section being

$$A\,y^2 + B\,x\,y + C\,x^2 + D\,y + E\,x + F = 0,$$

substituting for y, $r \sin\theta$, and for x, $r\cos\theta$, we get

$$A\,r^2 \sin^2\theta + B\,r^2 \sin\theta\cos\theta + C\,r^2\cos^2\theta + D\,r\sin\theta + E\,r\cos\theta + F = 0$$

the pole being at any point, put $r = \frac{k^2}{r'}$, or $\frac{1}{r'}$ for simplicity, and

$$\frac{A\sin^2\theta}{r'^2}+\frac{B\sin\theta\cos\theta}{r'^2}+\frac{C\cos^2\theta}{r'^2}+\frac{D\sin\theta}{r'}+\frac{E\cos\theta}{r'}$$
$$+F=0,$$

or multiplying by r^4,

$$A\,r'^2\sin^2\theta+B\,r'^2\sin\theta\cos\theta+C\,r'^2\cos^2\theta+D\,r'^3\sin\theta$$
$$+E\,r'^3\cos\theta+F\,r'^4=0$$

is the polar equation of the inverse conic section, its equation in rectangular coordinates being

$$A\,y^2+B\,x\,y+C\,x^2+D\,y\,(x^2+y^2)+E\,x\,(x^2+y^2)$$
$$+(x^2+y^2)^2=0.$$

1. If the focus be the pole, the distance from any point P to the focus is to its distance from the directrix in a constant ratio as e to 1.

Fig. 3.

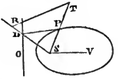
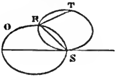


Fig. 4.



Now if we invert the line D O into a circle and the curve into the inverse focal ellipse whose equation is $r=k\,(1-e\cos\omega)$, we can construct the focal inverse ellipse by a circular directrix; (in fig. 4) let S be the pole (which is the focus), S O any circle passing through S; (in fig. 3) produce S P to T so that $ST=\frac{1}{SP}$, and S D to R so that $RS=\frac{1}{SD}$ (which is the same as to invert the curve and directrix) from the similar triangles R T S, D P S R T: R S:: D P: P S:: 1 : e

$$\because\ TRS=DPS=PSV.$$

Hence the circle circumscribing R S T is a tangent to S V at S; from this may be constructed the inverse focal conic section; for (in fig. 4) draw any chord S R to meet the circular directrix S R O, through S and R describe a circle S R T tangent to S V (the axis) at S, and inflect R T in a given ratio to R S, T is a point in the curve. As the cardioide is only a particular case of the focal conic section, this construction applies to it, making the ratio that of equality.

From the focal properties of conic sections we may deduce by inversion the following properties of the curve whose equation is $r=k\,(1-e\cos\omega.)$.

In the parabola the perpendicular from the focus on the tangent meets it in the vertical tangent. Hence in the cardioide, if a polar circle be drawn tangent to the curve, the locus of

the other extremity of the diameter passing through the pole is a circle passing through the cusp or pole and touching the curve at the opposite point, and consequently the locus of its centre is a circle.

In a conic section, if a point be taken inside the curve, and any chord be drawn, if we join the points in which it meets the curve with the focus, and also the given point with the focus, the product of the tangents of the half angles formed by those lines at the focus is constant; hence by inversion, if through a fixed point outside an inverse focal conic section we describe a polar circle, and join the points where it meets the curve with the pole, and also the given point with the pole, the product of the tangents of the half angles is constant.

In a conic section, if a chord subtend a constant angle at the focus, the envelope of the chord is a conic section with same focus and directrix; hence by inversion, if the arc of a polar circle contained between the points where it cuts the curve subtends a constant angle at the pole of a focal inverse conic, the envelope of this circle is an inverse focal conic with same pole and circular directrix.

If three tangents be drawn to a parabola, so as to form a triangle, the three angles and focus are in a circle; by inversion, if three circles be drawn through the pole of a cardioide touching the cardioide, the points of intersection are in a right line.

Every property, in fact, of a curve, with respect to any pole, has its analogous property in the inverse curve with respect to the same pole; to an asymptote in one, corresponds a circle passing through the pole and having its tangent at that point parallel to the asymptote, which the curve tends to approach as the radius diminishes; to a point of inflexion in one curve corresponds the property of the osculating circle at the conjugate or inverse point, passing through the pole; to a tangent in one corresponds a polar circle tangent to the other at the inverse point; to a cusp corresponds a cusp, and the osculating circle of the inverse curve is the inverse of that of the direct curve; so from the known properties of curves we can find the singular points of their inverse curves.

I shall not dwell any longer on those properties, as they are all obvious when the principle is explained. I shall merely show what Pascal's celebrated Theorem of the Hexagon inscribed in a conic section* becomes by inversion.

If in any inverse conic section six points be taken and six polar circles be described through each two consecutive points and the pole, the intersections of each opposite pair lie in a

[* See Phil. Mag. S. 3. vol. xxii. p. 168.—EDIT.]

circle passing through the pole; in the circle, the centre being the pole, this becomes a very remarkable theorem.

In two inverse curves the differential elements of the arcs are connected inthe following manner: $-d\,s' : d\,s :: r' : r$, or $d\,s' = \frac{k^4}{r^2}\,d\,s$; hence the differential element of the arc of a curve can be known when that of its inverse is known. This is remarkably connected with the theory of elliptic functions; the arc of an ellipse being represented by an elliptic function of the second kind, the arc of the curve formed by the intersections of the perpendiculars to the diameters of an ellipse through their extremities, by one of the first kind, I have found by this method that the arc of an inverse central ellipse is represented by $A\,\Pi\,(n,\,\phi) - B.F.\phi$, A and B being constants, and Π being an elliptic function of third kind with a circular parameter*, and $F\,\phi$ one of first kind; the amplitude ϕ being the angle by which the amplitude of the arc is measured in the ellipse from which it is generated; the x of the corresponding point of ellipse being $= a \sin\phi$, $y = b\cos\phi$. Hence from the general formula for the comparison of elliptic functions of the third kind (since if $\cos\sigma = \cos\phi\cos\psi - \sin\phi\sin\psi\sqrt{1-c^2\sin^2\sigma}$ $F\,\phi + F\,\psi - F\,\sigma = 0$), viz.

$$\Pi(n\phi)+\Pi(n\psi)-\Pi(n\sigma)=\frac{1}{\sqrt{\alpha}}\tan^{-1}\frac{n\sqrt{\alpha}\sin\psi\sin\phi\sin\sigma}{1+n-n\cos\psi\cos\phi\cos\sigma},$$

an infinite number of arcs of an inverse central ellipse may be found such that the difference between one measured from the vertex and the other between two other points shall be equal to a circular arc; and if the differenceof two arcs of an ellipse be equal to a right line, the difference of the arcs inverse to these shall be equal to a circular arc.

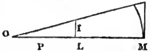
I will not trespass on your limits by proving this, it may be shown by the ordinary method; I will merely state the values of the constants; n the parameter $= \frac{a^2-b^2}{b^2}$, $A = \left(\frac{k^2\,a}{b^2} + \frac{k^2\,a\,c^2}{n\,b^2}\right)$, $B = \frac{k^2\,c^2\,a}{n\,b^2}$, a and b being the axes of the common ellipse, c its eccentricity, k the modulus of the inverse curve, or the constant equal to the rectangle under the coincident radii of the two curves. By discussing the formula above given for the comparison of elliptic functions of the third species, substituting for n and α $\left(\alpha \text{ is } (1+n)\,(1+\frac{c^2}{n})\right)$,

* This is not as general as I could wish, as there is a relation between the modulus and parameter which properly should be independent.

when the arcs are measured from the extremities of the axes, I come to the following theorem.

The difference of the arcs of an inverse ellipse, one measured from the end of the major, the other of the minor axis, and whose amplitudes fulfil the condition $b \tan \phi \tan \psi = a$ (a and b being the axes of the direct ellipse), is equal to the arc of a circle, which may be found by the following construction:—let I be the intercept between the foot of the perpendicular from the centre on the tangent of the direct ellipse and the point of contact, P the perpendicular from the centre on the line joining the extremities of the axes of the direct ellipse, and L the line joining the extremities of the axes of the inverse ellipse; then taking a line = P, raising at its extremity a perpendicular = I, and producing the line P until the whole line produced = L, with the common extremity O as centre, and L as radius, describe a circle, the arc of this circle intercepted between the other extremity, M, and the line joining O with the end of the perpendicular I, is the difference of the required arcs. The analogy of this theorem to Fagnani's with regard to the direct ellipse (by which the difference of the corresponding arcs of it is I) is obvious. The area of the inverse ellipse is an arithmetic mean between the areas of the circles described on its axes.

Fig. 5.



To apply the inverse method to surfaces I will state the following principles: if one surface be inverse to another, a tangent plane being drawn at one point, the tangent plane at the inverse point is had by bisecting the line joining the points by a plane perpendicular to this line, and through the line where it cuts the tangent plane to the first surface, and the inverse point we draw a plane; it is a tangent plane at the inverse point: this is readily seen, as if through the common radius we draw any plane cutting the surfaces in two curves, these curves are inverse, and the construction which I gave for the tangents at inverse points makes this construction evident. Hence the normals at inverse points of surfaces are in the same plane and equally inclined to the common radius.

From this construction for the tangent plane, it follows that if two surfaces cut at right angles their inverse surfaces cut at right angles. Hence if we describe the developable surfaces formed by the tangent planes and normals at the points of a line of curvature, since these surfaces cut at right angles their inverse surfaces cut at right angles at the inverse points of the line of curvature, but the surface formed by the tangent planes to the inverse surface at those points is touched by the inverse

of the corresponding surface in the first, and similarly the surface of normals by inverse of that in original surface; hence it may be seen that the normals to the inverse surface along the inverse points of a line of curvature meet consecutively, or the inverse of a line of curvature on a surface is the line of curvature of the inverse surface; or if the line of curvature of a surface be known, that of its inverse surface is had by describing a cone with the pole as vertex and passing through the line of curvature on direct surface, the line in which it pierces the inverse surface is a line of curvature.

Hence the umbilici of one surface correspond to the umbilici of the second; and in general to a tangent plane corresponds a polar sphere, and to all the singular points of one surface correspond singular points of the second.

From the known theorems of surfaces of the second order may be deduced numberless theorems of surfaces of the fourth order by inversion, similarly as in plane curves. I shall confine myself to the inverse of the central ellipsoid, which is Fresnel's surface of elasticity in the wave theory of light.

1. By the construction for the tangent plane to an inverse surface, the tangent plane to the surface of elasticity may be found from knowing the tangent plane of the ellipsoid.

2. The lines of curvature on that surface may be found by producing the cone passing through the lines of curvature on the ellipsoid to meet it, but

3. The intersection of a confocal ellipsoid and hyperboloid determines the line of curvature on either, as they cut at right angles; hence as the equation of the inverse ellipsoid is $\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=(x^2+y^2+z^2)^2$, if two inverse central surfaces of the second order have their constants connected by the condition $a^2-a'^2=b^2-b'^2=c^2-c'^2$, and they intersect, they cut at right angles, and their intersection is the line of curvature on either.

4. By subtracting the equations

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}+\frac{z^2}{c^2}=(x^2+y^2+z^2)^2, \text{ and}$$
$$\frac{x^2}{a'^2}+\frac{y^2}{b'^2}+\frac{z^2}{c'^2}=(x^2+y^2+z)^2,$$

we get in the case above mentioned,

$$\frac{x^2}{a^2\,a'^2}+\frac{y^2}{b^2\,b'^2}+\frac{z^2}{c^2\,c'^2}=0,$$

the equation to a cone of the second order: the intersection

of this cone with the surface inverse to the ellipsoid is the line of curvature.

5. By putting $x^2 + y^2 + z^2 =$ a constant, we find $\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} =$ const., or the intersection of the surface of elasticity and concentric sphere is a spherical conic, since it is the same as the intersection of the surface of second order and sphere.

6. The circular sections of the surface of elasticity correspond to those of the ellipsoid, and the umbilici of either are found by the intersection of the diameters conjugate to the circular section of the ellipsoid, as at the umbilici the ultimate section must be a circle, and therefore parallel to the circular sections.

7. As to the rectilinear generatrices of the hyperboloid of one sheet correspond circular sections in the inverse hyperboloid, the latter has an infinite number of circular sections passing through the centre, but only two whose centre is at that point.

8. Hence the inverse hyperboloid of one sheet may be described by a moveable circle passing through a given point and moving on three others passing through the same point, which only cut in that point, and which neither lie in the same plane nor are circles of same sphere.

9. From the property that the sum of the squares of the reciprocals of three radii vectores at right angles to each other is constant in the ellipsoid, it follows that the sum of the squares of three rectangular semidiameters in the surface of elasticity is constant.

10. As the locus of the feet of perpendiculars from the centre on tangent planes of an ellipsoid is a surface of elasticity; by inversion, the locus of centres of spheres tangent to a surface of elasticity and passing through the centre is an ellipsoid.

11. From 8 and 5 it follows, that if planes pass through the centre of a surface of elasticity and cut out sections of a constant area, they envelope a cone of the second order, since the sum of the squares of the axes of the section is constant.

The foregoing are a few of the general theorems that may be deduced by the method I propose; they furnish a new instance of the duality that Chasles and others have remarked between the properties of figures; but it is superior to any hitherto proposed, as we can by it arrive at once at the properties of curves of higher orders which surpass our present power of analysis, from those of known curves; as from the known pro-

perties of curves of the second order, we come to those of the fourth with regard to polar circles, so by deducing those of the second order with regard to these latter, we might arrive at the properties of the higher curves with regard to lines. I shall not trespass on your limits any further by noticing new properties, many of which I have deduced in the paper before alluded to. I shall merely show the application of this method to physical investigation by two simple instances.

1. Since the ultimate elements of two inverse surfaces corresponding to each other are inversely placed with regard to the common radius, by describing an infinitesimal cone having these elements for their bases, $\frac{m}{d^2} = \frac{m'}{d'^2}$, m and m' being the masses of the bases, and d and d' the distances; hence the attraction of two infinitesimal elements resolved in any direction are the same, or the whole attractions of the corresponding parts of two inverse surfaces on the common pole is the same. The application of this to the plane and sphere is obvious.

Fig. 6.

2. The second theorem I shall state regards the wave-theory of light. It is stated by Sir John Herschel in his Essay on Light, that the equation for determining the velocity of a wave perpendicular to a given plane $z = mx + ny$, is

$$(V^2 - a^2)(V^2 - b^2) + m^2 (V^2 - b^2)(V^2 - c^2) + n^2 (V^2 - a^2)(V^2 - c^2) = 0,$$

and that this equation is had by an elimination which he states to be very complicated: it can be had at once by the following geometrical method.

Taking with him the equation of the surface of elasticity to be

$$R^4 = a^2 x^2 + b^2 y^2 + c^2 z^2;$$

if we find the intersection of this with the concentric sphere, we get

$$x^2 (a^2 - r^2) + y^2 (b^2 - r^2) + z^2 (c^2 - r^2) = 0;$$

but if we put $r =$ to a constant, this represents a cone of second order. Now if we draw a tangent plane to this cone through the centre, the line of contact must be the axis of the curve cut out as the tangent to the sphere whose radius at r is perpendicular to it at its extremity; but this is also contained in the tangent plane, and therefore the intersection of these two planes is a tangent to the section perpendicular to its diameter, which is therefore an axis, but identifying the equation of the tangent plane to the cone, viz. $x x' (a^2 - r^2) + y y' (b^2 - r^2) + z z' (c^2 - r^2) = 0$, with the plane $l x + m y + n z = 0$, $l = x' (a^2 - r^2)$ $m = y' (b^2 - r^2)$, &c., x' y' z' being the coor-

dinates of the extremity of the diameter, but $x'^2\,(a^2-V^2) + y'^2\,(b^2-V^2) + z'^2\,(c^2-V^2) = 0$,

$$\therefore \frac{l^2}{(a^2-V^2)} + \frac{m^2}{b^2-V^2} + \frac{n^2}{c^2-V^2} = 0,$$

which is the equation at which he arrives when we make $l = 1$.

I remain, Gentlemen, yours, &c.,

Trinity College, Dublin, JOHN WM. STUBBS.
Jan. 31, 1843.

XLIII. *On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat.* By J. P. JOULE, *Esq.*

[Continued from p. 276.]

Part I. *On the Calorific Effects of Magneto-Electricity.*

I NOW proceeded to consider the heat evolved by voltaic currents when they are counteracted or assisted by magnetic induction. For this purpose it was only necessary to introduce a battery into the magneto-electrical circuit: then by turning the wheel in one direction I could oppose the voltaic current; or, by turning in the other direction, I could increase the intensity of the voltaic- by the assistance of the magneto-electricity. In the former case the apparatus possessed all the properties of the electro-magnetic engine; in the latter it presented the reverse, viz. the *expenditure* of mechanical power.

No. 7.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	Temperature of Water. After.	Loss or Gain.
May 19.	Circuits complete.	600	22° 40′	57°·43	1°·03 −	55°·62	57°·18	1°·56 gain.
	Circuits broken.	600	0 0	57·45	0·41 −	57·08	57·00	0·08 loss.
May 20.	Circuits complete.	600	20 45	59·40	0·08 −	58·65	60·00	1·35 gain.
	Circuits broken.	600	0 0	59·40	0·51 +	60·00	59·83	0·17 loss.
	Circuits complete.	600	23 0	59·10	1·29 +	59·78	61·00	1·22 gain.
	Mean circuits complete.	600	22 8	....	0·06 +	....	....	1·38 gain.
	Mean circuits broken.	600	....	....	0·05 +	....	....	0·12 loss.
	Corrected Result.	600	22° 8′ = 0·864 of current.					1·50 gain.

In the preceding series I used the steel magnets previously described as the inductive force: and I had two of the large Daniell's cells in series, arranged so as to pass a current of electricity through the revolving electro-magnet and galvanometer. The wheel was turned in the direction which it would have taken had the friction been sufficiently reduced to allow of the motion of the apparatus without assistance. The precaution of interpolating the experiments was again adopted.

I give another series, in which everything else remaining the same, the direction of revolution was reverse, so as to *increase* the intensity of the voltaic electricity by superadding that of the magneto-electricity.

No. 8.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
May 23.	Circuits complete.	600	30° 15′	60°·55	0°·19 +	59°·30	62°·18	2°·88 gain.
	Circuits broken.	600	0 0	62·28	0·48 +	62·92	62·60	0·32 loss.
May 24.	Circuits complete.	600	29 40	60·90	0·03 —	59·50	62·25	2·75 gain.
	Circuits broken.	600	0 0	59·50	0·0	59·50	59·50	0
May 26.	Circuits complete.	600	29 50	61·85	0·18 —	60·33	63·02	2·69 gain.
	Circuits broken.	600	0 0	60·90	0·49 —	60·50	60·33	0·17 loss.
	Mean Circuits complete.	600	29 55	....	....	....	....	2·77 gain.
	Mean Circuits broken.	600	....	....	....	....	....	0·16 loss.
	Corrected Result.	600	29° 55′ = 1·346 of current.					2·93 gain.

Dismissing the steel magnets, which did not appear to have lost any of the magnetic virtue which they possessed when newly made, I now substituted for them the large stationary electro-magnet, excited by eight of the Daniell's cells arranged in a series of four double pairs. The revolving electro-magnet completed, as before, a circuit containing the galvanometer and 2 of Daniell's cells in series. The motive power of the apparatus was now so great that it would revolve rapidly in spite of very considerable friction. In order to give the re-

quisite velocity it was necessary however to assist the motion by the hand.

No. 9.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
May 31, P. M.	Circuits complete.	600	16°	62·50°	0·11°—	62·00°	62·78°	0·78° gain.
	Circuits broken.	600	0	63·00	0·23—	62·73	62·82	0·09 gain.
June 1, A. M.	Circuits complete.	600	14	62·65	0·11—	62·18	62·90	0·72 gain.
	Circuits broken.	600	0	63·15	0·20—	62·90	63·00	0·10 gain.
	Mean Circuits complete.	600	15	....	0·11—	....	....	0·75 gain.
	Mean Circuits broken.	600	....	....	0·21—	....	....	0·095 ga.
	Corrected Result.	600	15° = 0·543 of current.					0·68 gain.

The following series of results was obtained with the same apparatus, by turning the wheel in the opposite direction.

No. 10.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
June 2, A. M.	Circuits complete.	600	35° 10′	65·38°	0·62°+	63·25°	68·75°	5·50° gain.
	Circuits broken.	600	0 0	64·73	0·75+	65·51	65·45	0·06 loss.
June 3, A. M.	Circuits complete.	600	37 10	65·10	1·40+	63·33	69·66	6·33 gain.
	Circuits broken.	600	0 0	64·93	1·23+	66·28	66·05	0·23 loss.
	Mean Circuits complete.	600	36 10	....	1·01+	....	....	5·915gain
	Mean Circuits complete.	600	....	....	0·99+	....	....	0·145 loss
	Corrected Result.	600	36° 10′ = 1·845 of current.					6·06 gain.

I give two series more, in which only one cell was connected with the revolving electro-magnet, and the revolution was in the direction of the attractive forces. The magneto-electricity

No. 11.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
June 3, P. M.	Circuits complete.	350	0	64°·02	0°·38−	63°·57	63°·72	0°·15 gain.
	Circuits broken.	350	0	63·75	0·02−	63·73	63·73	0
	Circuits complete.	400	0	63·80	0·02−	63·70	63·86	0·16 gain.
	Circuits broken.	400	0	64·35	0·08−	64·27	64·27	0
	Mean Circuits complete.	375	0	....	0·20−	....	....	0·155 gain
	Mean Circuits broken.	375	0	....	0·05−	....	....	0
	Corrected Result.	375	0					0·12 gain.

No. 12.

		Revolutions of Electro-Magnet per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	After.	Loss or Gain.
June 5, A. M.	Circuits complete.	600	9° 40′	60°·40	0°·16−	60°·02	60°·47	0°·45 gain.
	Circuits broken.	600	0	60·64	0·17−	60·47	60·47	0
	Corrected Result.	600	9° 40′=0·34	current in the opposite direction.				0·45 gain.

was so intense, at a velocity of 600 per minute, as to overpower the intensity of the single cell, causing the needle to be permanently and steadily deflected to between 9° and 10° in the opposite direction. The command of the magneto-electricity over the voltaic current arising from one cell was beautifully illustrated by the sparks at the commutator. Turning

slowly, they were bright and snapping*; increasing the rapidity of revolution, they decreased in brightness: until at a velocity of about 370 per minute they ceased altogether. They were plainly visible again when the velocity reached 600 per minute.

The results of the preceding series of experiments are collected in the following table along with theoretical results calculated in precisely the same manner as those of Table I. The correction for heat evolved by the iron of the revolving electro-magnet is estimated at 0°·18, the product of 0°·28 by $\left(\frac{4}{5}\right)^2$; because in the above experiments the large electro-magnet was excited by $\frac{4}{5}$ of the battery used when 0°·28 was obtained. No correction is needed for the series in which the steel magnets were used, because they remained in their places during the interpolating experiments.

TABLE II.

Series of Experiments.	Current Magneto-Electricity.	Heat actually evolved.	Correction for Currents in the Iron.	Corrected Heat.	Squares of Numbers proportional to those in column 2.	Heat due to Voltaic Currents of the intensities given in col. 2.	The Numbers of column 7 multiplied by $\frac{4}{3}$.
No. 7.	0·864	1°·50	0°	1°·50	1°·291	0°·954	1°·272
No. 8.	1·346	2·93	0	2·93	3·133	2·316	3·088
No. 9.	0·543	0·68	0·18	0·50	0·510	0·377	0·503
No. 10.	1·845	6·06	0.18	5·88	5·886	4·351	5·801
No. 11.	0	0·12	0·18	0·06	0	0	0.
No. 12.	0·340	0·45	0·18	0·27	0·200	0·148	0·197
1.	2.	3.	4.	5.	6.	7.	8.

In all these experiments, as well as in those collected in Table I, the time occupied by the platinum wires in crossing the divisions of the commutator was found to be exactly $\frac{1}{4}$ of that occupied by an entire revolution. Hence the multiplication by $\frac{4}{3}$ in order to obtain true theoretical results on the supposition that the current flows uniformly during $\frac{3}{4}$ of a revolution. It will be observed that these theoretical results

* The most splendid sparks are obtained when the voltaic is assisted by the magneto-electricity, by turning an electro-magnetic engine in a direction contrary to the attractive forces.

are not so much inferior to the experimental results of column 5 as they were in Table I. The principal reason of this arises from the mixture of the constant effect of the battery with the variable magneto-electrical current, as will be readily seen on inspecting figs. 6 and 7, the former of which represents the currents in series No. 9; the latter, those in series No. 10. The dotted rectangles *a b c d*, &c., represent the constant effect of the battery of two cells, which is in one instance diminished, in the other increased by the magneto-electricity.

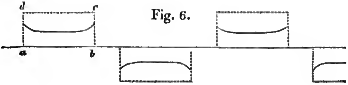


Fig. 6.

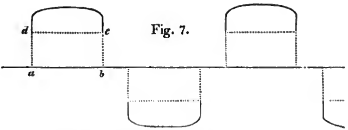


Fig. 7.

On comparing columns 6 and 8 with column 5, it is manifest that the law of the square of the electric current still obtains, and is not affected either by the assistance or resistance which the magneto-electricity presents to the voltaic current. Now the increase or diminution of the chemical effects occurring in the battery during a given time is proportional to the magneto-electrical effect, and the heat evolved is always proportional to the square of the current; therefore the heat due to a given chemical action is subject to an increase or to a diminution directly proportional to the intensity of the magneto-electricity assisting or opposing the voltaic current.

We have therefore in magneto-electricity an agent capable by simple mechanical means of destroying or generating heat. In a subsequent part of this paper I shall make an attempt to connect heat with mechanical power in absolute numerical relations. At present we shall turn to a question intimately connected with the previous investigations; and which indeed has already been partly developed.

On the Heat evolved by a Bar of Iron rotating under Magnetic Influence.

Having removed the small electro-magnet from out of the tube of the revolving piece, I fixed in its stead, in the centre of the tube, a solid cylinder of iron 8 inches long and $\frac{3}{4}$ of an inch in diameter. The tube was then, as before, filled with water and rotated for a quarter of an hour between the poles of the large electro-magnet. In the first experiments the electro-magnet was excited by ten cells in a series of five double pairs, a galvanometer being included in the circuit in order to indicate the electric force applied. It was of course placed, as before, so as not to be affected by the powerful attraction of the electro-magnet. The precaution of interpolating the experiments was adopted as usual.

No. 13.

		Revolutions of the Bar per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	Temperature of Water. After.	Gain or Loss.
June 17, P.M.	Electro-magnet in action.	600	70° 55′	67°·38	0°·35—	66°·27	67°·80	1°·53 gain
	Battery contact broken.	600	...	67·60	0·23+	67·77	67·90	0·13 gain
	Electro-magnet in action.	600	70 45	67·85	0·67+	67·85	69·20	1·35 gain
	Battery contact broken.	600	...	68·92	0·30+	69·18	69·27	0·09 gain
	Mean, Electro-magnet in action.	600	70 50	...	0·16+	...	...	1·44 gain
	Mean, Battery contact broken.	600	...	...	0·26+	...	...	0·11 gain
	Corrected Result.	600	70°·50 = 9·85 current.					1·31 gain

Everything else remaining the same, I now used a battery of six cells arranged in a series of three double pairs to excite the electro-magnet.

No. 14.

		Revolutions of the Bar per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	Temperature of Water. After.	Gain or Loss.
June 19, A.M.	Electro-magnet in action.	600	64° 10′	65°·42	0°·17—	65°·00	65°·50	0°·50 gain
	Battery contact broken.	600	...	65·55	0·10—	65·48	65·42	0·06 loss
	Electro-magnet in action.	600	64 10	65·42	0·17+	65·33	65·86	0·53 gain
	Battery contact broken.	600	...	65·75	0·03+	65·80	65·77	0·03 loss
	Mean, Electro-magnet in action.	600	64 10	...	...	...	...	0·515 gain
	Mean, Battery contact broken.	600	...	...	0·03—	...	...	0·045 loss
	Corrected Result.	600	64°·10′ = 6·77 current.					0·56 gain

I give another series obtained with a battery of two cells in series.

No. 15.

		Revolutions of the Bar per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water. Before.	Temperature of Water. After.	Gain or Loss.
June 19, P.M.	Electro-magnet in action.	600	54°	63°·65	0°·43—	63°·12	63°·32	0°·20 gain
	Battery contact broken.	600	...	63·80	0·38—	63·40	63·45	0·05 gain
	Electro-magnet in action.	600	54	63·75	0·20—	63·45	63·65	0·20 gain
	Battery contact broken.	600	...	64·07	0·37—	63·68	63·73	0·05 gain
	Mean, Electro-magnet in action.	600	54	...	0·32—	...	...	0·20 gain
	Mean, Battery contact broken.	600	...	...	0·38—	...	...	0·05 gain
	Corrected Result.	600	54° = 4·17 current.					0·16 gain

The results of the preceding experiments are collected in the following table:—

TABLE III.

Series of Experiments.	Current employed in exciting the Electro-Magnet.	Heat Evolved.	Square of Numbers proportional to those of Column 2.
No. 13.	9·85	1°·31	1·2290
No. 14.	6·77	0·56	0·5807
No. 15.	4·17	0·16	0·2203
1	2	3	4

It was discovered by Prof. Jacobi, and by myself also one or two months afterwards*, that the attraction of electro-magnets, either towards one another or for their armatures, is (below the point of saturation) proportional to the square of the electric force. The *magnetism* in an electro-magnet is therefore simply as the electric force. Consequently the numbers in column 2 are proportional to the magnetic virtue of the electro-magnet. But on comparing columns 3 and 4 together, it will be seen that the heat evolved is as the square of the electricity. Therefore *the heat evolved by a revolving bar of iron is proportional to the square of the magnetic influence to which it is exposed.*

After the preceding experiments there can be no doubt that heat would be evolved by the rotation of non-magnetic substances, in proportion to their conducting power, Dr. Faraday having proved the existence of currents in such circumstances, and that their quantity is proportional, *cæteris paribus*, to the conducting power of the body in which they are excited. I have not made any experiments on this subject, but in the next part we shall have occasion to avail ourselves of the good conducting power of copper, in conjunction with the magnetic virtue of the bar of iron, in order to obtain a maximum result from the revolution of a metallic bar.

[To be continued.†]

* Annals of Electricity, vol. iv. p. 131. Jacobi and Lenz communicated their report to the St. Petersburgh Academy in March 1839,—two months previous to the date of my paper.

[† Mr. Joule requests us to insert the following *erratum* in the former portion of his paper: p. 274, *for* B C, C D, &c., *read* B C, D E, &c.—EDIT.]

XLIV. *On the so-called Calorotypes, with Animadversions on the Papers of* Mr. Hunt *and* Prof. Draper *lately published in the Philosophical Magazine.* *By* Prof. Ludwig Moser *.

I WAS not a little surprised to find in vol. lviii. of these *Annalen*, an article by Mr. Robert Hunt † of Falmouth on Thermography, and one on Calorotypes by M. Knorr, in which are communicated as discoveries facts which I had previously described at length in the same work. Mr. Hunt sets out from my first Memoir on Vision ‡, in which I demonstrated the existence of invisible light; he repeats the experiments which he read § in this Memoir, and gives them out as his own discovery. I cannot name a single experiment communicated by either of the authors which I had not previously described, except it be that the one employs a jasper instead of an agate, which I used, or a bronze medallion instead of a piece of copper coin.

In my Memoir on Vision, &c. of May 1842 (vol. lvi. of these *Annalen*), I communicated at page 206 [Scient. Mem. vol. iii. p. 43] the fact, that when any body is warmed it depicts itself on metallic or glass plates; that the same also occurs when the plate is warmed instead of the body. These experiments led me at first to believe that heat had some part in the production of these images, and Mr. Hunt, as also M. Knorr, who have repeated them, have stopt short at this erroneous view. Not so with me; for the following page described the same experiment made without the intervention of any artificial temperature, adding the following observations:—

"The action of light was therefore imitated and extended by my experiments; and indeed, as it appeared, by the employment of unequal temperatures. But this latter view could not possibly be long entertained; it is only necessary to observe one of the above-described images, if well executed, in order to be convinced that such representations, in which the finest lines of the original may often be traced, could not possibly be produced by differences of temperature, more particularly on a thin, well-conducting plate of metal. Moreover, the variety of the substances em-

* From Poggendorff's *Annalen*, vol. lix. part 1. Translated and communicated by William Francis, Ph.D.

† The paper referred to is a translation of Art. LXXXI. in the Phil. Mag. for Dec. 1842.—Edit.

‡ A translation of the whole of this Memoir, and also of those on Latent Light and on Invisible Light, by M. Moser, were published in Part XI. of Taylor's Scientific Memoirs, in Feb. 1843.

§ Mr. Hunt's paper, as just intimated, appeared in the Philosophical Magazine for December 1842. The first and very imperfect notice in this country of M. Moser's investigations was communicated at the Meeting of the British Association in June of the same year. It related solely to experiments and views detailed in the first of the three Memoirs.—Edit.

ployed forms a great objection to this view of the subject. It was therefore necessary to try whether the same phænomena could not be produced without the application of heat, and in this I succeeded." [Scientific Memoirs, vol. iii. p. 443.]

In the Addendum to this Memoir [Scient. Mem. vol. iii. p. 459], in which I advanced the hypothesis that the depicting of the bodies was due to a light proper to themselves, I have spoken of the auxiliary influence of heat in such a manner as will I hope settle this point. Bodies become luminous when heated; that is, they emit light of the refrangibility of ordinary light. After this it will hardly be admitted that this emission of light takes place suddenly; besides, the experiments prove the contrary; they show that light is radiated at all temperatures from bodies, that its intensity increases and its refrangibility decreases as the temperatnre becomes higher. According to my experiments the invisible rays of light pass very readily through aqueous solutions of various kinds, and through different oils, but they decidedly do not pass through the thinnest plates of glass, mica, amber or rock salt. [I have only recently made some experiments with the latter substance.] But if the temperature be raised, they pass very readily both through glass and mica, which is perfectly in accordance with the supposition that their intensity is increased and their refrangibility approximated to those of visible light. [Substances such as white glass, mica, &c., consequently lose the character of perfect transparency; they retain it only for a certain group of rays of light.]

Should it be concluded from the influence of heat on light,—corresponding so closely in its other properties to other physical forces,—that light and heat are identical, then let us see further on how the remaining phænomena are to be explained. I would merely impress on the reader not to be led astray by the mass of proofs which might perhaps be enumerated in favour of such an identity; they are all of them nothing more than variations upon the old fact of the incandescence of bodies at an elevation of temperature. With this I may at present take my leave of Mr. Hunt; the very title of his paper conveys the opinion of its being dependent on the influence of heat, and he has not even entered into the subject so far as he read it in my first Memoir. As I have stated above, he has not devised a single new experiment, for even those which appear to him sufficiently important to be adopted as the running head to his paper, "Art of copying engravings from paper on metallic plates by means of heat," will be found nearly word for word in these *Annalen*, vol. lvii. p. 570*.

* Published in 1842.

It is that experiment in which I caused a seal to depict itself on mercury with which a pure or silvered copper-plate had been coated, and afterwards produced the image in the iodine vapours.

I now turn to the calorotypes of M. Knorr. As soon as I had found from my experiments the fact that the actions of the bodies were manifested without any elevation of temperature, I operated at the ordinary temperature and never employed heat, for the phænomenon appeared already to belong to a somewhat complicated class, which needed not to be rendered more intricate by the introduction of any foreign force. In one case only did I depart from this rule, and that was in order to determine the colour of the latent light of oxygen. Since the affinity of several metals for this gas is increased by heat, I warmed plates of copper and brass, on which the invisible rays had acted, and on their becoming iridescent, I obtained the images by means of the various colouring. I communicated these results to several persons about the 18th Sept. 1842, and among others to the Editor of these *Annalen* (Prof. Poggendorff), who had the kindness to bring them before the notice of the Berlin Academy of Sciences, and to cause their publication in the Monthly Proceedings*. Now these are the calorotypes of M. Knorr, only, as I conceive, obtained in a more advantageous manner; for M. Knorr heats the plate with the body to be depicted on it until the former becomes iridescent. The image which is formed under these circumstances I have long been acquainted with, but even now in my opinion it affords no proof. If, for instance, a body is placed on the plate, at some points it will be in contact, at others not; the oxygen to which the iridescence must be ascribed will be present in some places in sufficient quantity, or have free access, at other points not; moreover, some parts of the plate will acquire a higher temperature on being heated than the others, so that if after all we see the image of a body on a plate, it may be owing to several circumstances. In my experiments I have avoided these, for I allow the body in contact or at a distance to act first on the metallic plates by its peculiar light, and then heat the plate uniformly, the body to be depicted being absent, and the oxygen having free access. I may therefore assert that the calorotypes of M. Knorr are no new discovery, and that they do not in the least alter the state of the case; for the conclusions which might be drawn from the images produced by iridescence I have already communicated, while M. Knorr contents himself with the fact.

* See Poggendorff's *Annalen*, vol. lviii.

As the question as to the identity of light and heat is now frequently discussed, I will communicate a fact on this subject which in my opinion will prove decisive: it had escaped me when writing the paper on the question of the identity contained in these *Annalen*, vol. lviii. p. 105. Heat, as is well known, is emitted from bodies to which it has been conveyed, presupposing that it was not employed to produce a chemical change in them: the light, on the contrary, which effected that peculiar change on the surface of bodies which subsequently is best rendered perceptible by the condensation of vapours, is not again radiated, and it must therefore be assumed that it has become extinct with this action. I have made numerous experiments to transfer the effect of the light from one plate to the other; sometimes I took iodized silver plates with a not yet perceptible image from the camera obscura, placed them in contact for a short or long period with other iodized plates, or with plates of other metals; sometimes I employed for the same purpose metallic plates on which the image produced by imperceptible light was in a far more advanced state. As soon as I became acquainted with the behaviour of the oils I tried them, and separated the two plates employed in the experiment by a layer of oil; in no case have I succeeded in detecting even a trace of transfer of an image on to another plate. The light which has produced its effect radiates accordingly no more.

Now what shall we say to the experiments of Dr. Draper, in which this re-radiation is assumed as something self-evident, and upon which indeed is principally founded a theory of tithonic rays? I may inform those philosophers who are not acquainted with the investigations of Dr. Draper, that these new kind of rays are nothing else than the chemical rays which have often been said to have been discovered in the solar spectrum. Dr. Draper has just as much discovered them, as the imperceptible and latent light; both of which species of light he likewise lays claim to, without there having been the least mention of them in his papers. But to keep to the subject, it is impossible not to be astonished at a physicist who conceives he has discovered a new force—the tithonic, and asserts that it radiates without even having made one single experiment to prove this. Dr. Draper knows no more than what every person who has been engaged in experiments on light is aware of, that the image of an iodized silver plate, as it comes from the camera obscura, disappears after a time, and can no more be made to appear in the vapours of mercury. Does it hence follow that the image radiates from the plate? This is so little the case, that, according to my experience,

the disappearance of the image must rather be ascribed to a peculiar action of the oxygen of the atmosphere, on which subject I recently made a preliminary communication to Sir David Brewster and Professor Magnus, and intend shortly to publish the details in these *Annalen*. Dr. Draper is so convinced of the radiation of these images, that he pretends to preserve them by covering them, and in this scientific manner has discovered specific light analogous to specific heat!

Königsberg, 30th April, 1843.

XLV. *On an Expression for the Numbers of* Bernoulli, *by means of a Definite Integral; and on some connected Processes of Summation and Integration. By* Sir William Rowan Hamilton, *LL.D., P.R.I.A., Member of several Scientific Societies at Home and Abroad, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

THE following analysis, extracted from a paper which has been in part communicated to the Royal Irish Academy, but has not yet been printed, may interest some readers of the Philosophical Magazine.

1. Let us consider the function of two real variables, m and n, represented by the definite integral

$$y_{m,n} = \int_0^\infty d\,x \left(\frac{\sin x}{x}\right)^m \cos n\,x; \quad . \quad . \quad (1.)$$

in which we shall suppose that m is greater than zero; and which gives evidently the general relation

$$y_{m,-n} = y_{m,n}.$$

By changing m to $m+1$; integrating first the factor $x^{-m-1}\,d\,x$, and observing that $x^{-m}\sin x^{m+1}\cos n\,x$ vanishes both when $x=0$, and when $x=\infty$; and then putting the differential coefficient $\frac{d}{d\,x}(\sin x^{m+1}\cos n\,x)$ under the form

$$\tfrac{1}{2}\sin x^m\{(m+1+n)\cos(n\,x+x)+(m+1-n)\cos(n\,x-x)\};$$

we are conducted to the following equation, in finite and partial differences,

$$2\,m\,y_{m+1,n} = (m+1+n)\,y_{m,n+1} + (m+1-n)\,y_{m,n-1}; \quad (2.)$$

and if we suppose that the difference between the two variables on which y depends is an even integer number, this equation takes the form

$$m\, y_{m+1, m+1-2k} = (m+1-k)\, y_{m, m+2-2k} + k\, y_{m, m-2k}. \quad . \quad (3.)$$

The same equation in differences (2.) shows easily that

$$y_{m+1, n} = 0, \quad \text{when } n = \text{or} > m+1,$$
$$\text{if } y_{m, n-1} = 0, \quad \text{when } n - 1 > m;$$

but, by a well-known theorem, which in the present notation becomes

$$y_{1,0} = \frac{\pi}{2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

it is easy to prove, not only that

$$y_{1,1} = y_{1,-1} = \frac{\pi}{4}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

but also that

$$y_{1,n} = 0, \text{ if } n^2 > 1; \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

we have therefore, generally, for all whole values of $m > 1$, and for all real values of n,

$$y_{m,n} = 0, \text{ unless } n^2 < m^2. \quad . \quad . \quad . \quad (7.)$$

2. If then we make

$$T_m = \Sigma\, y_{m, m-2k} (-t)^k, \quad . \quad . \quad . \quad . \quad (8.)$$

the sign Σ indicating a summation which may be extended from as large a negative to as large a positive whole value of k as we think fit, but which extends at least from $k = 0$ to $k = m$, m being here a positive whole number; this sum will in general (namely when $m > 1$) include only $m - 1$ terms different from 0, namely those which correspond to $k = 1, 2, \ldots m - 1$; but in the particular case $m = 1$, the sum will have two such terms, instead of none, namely those answering to $k = 0$ and $k = 1$, so that we shall have

$$T_1 = y_{1,1} - y_{1,-1}\, t = \frac{\pi}{4}(1 - t). \quad . \quad . \quad . \quad (9.)$$

Multiplying the first member of the equation in differences (3.) by $(-t)^k$, and summing with respect to k, we obtain $m\, T_{m+1}$, m being here any whole number > 0. Multiplying and summing in like manner the second member of the same equation (3.), the term $m\, y_{m, m+2-2k}$ of that member gives $-m\, t\, T_m$, because we may change k to $k+1$ before effecting the indefinite summation; $k\, y_{m, m-2k}$ gives $t \frac{d}{dt} T_m$; and $(1 - k)\, y_{m, m+2-2k}$ gives $t^2 \frac{d}{dt} T_m$; but

$$-m\, t\, T_m + (t + t^2) \frac{d}{dt} T_m = (1 + t)^{m+1} \frac{t\, d}{dt} (1 + t)^{-m}\, T_m;$$

therefore

$$m\,(1+t)^{-m-1}\,\mathrm{T}_{m+1} = \frac{d}{d\log t}\,(1+t)^{-m}\,\mathrm{T}_m. \quad . \ (10.)$$

This equation in mixed differences gives, by (9.),

$$\mathrm{T}_m = \frac{\pi}{4}\,\frac{(1+t)^m}{1\,.\,2\,.\,3\ldots(m-1)}\left(\frac{d}{d\log t}\right)^{m-1}\frac{1-t}{1+t}; \quad . \ (11.)$$

the factorial denominator being considered as $= 1$, when $m = 1$, as well as when $m = 2$. If $m > 1$, we may change $\frac{1-t}{1+t}$ to $\frac{2}{1+t}$, from which it only differs by a constant; and then by changing also t to e^h, and multiplying by $\frac{2}{\pi}$, we obtain the formula:

$$\left.\begin{aligned} &\frac{(e^h+1)^m}{1\,.\,2\,.\,3\ldots(m-1)}\left(\frac{d}{dh}\right)^{m-1}(e^h+1)^{-1} \\ &= \frac{2}{\pi}\,\Sigma_{(k)\,1}^{m-1}\int_0^\infty dx\left(\frac{\sin x}{x}\right)^m(-e^h)^k\cos(mx-2kx); \end{aligned}\right\}(12.)$$

which conducts to many interesting consequences. A few of them shall be here mentioned.

3. The summation indicated in the second member of this formula can easily be effected in general; but we shall here consider only the two cases in which m is an odd or an even whole number greater than unity, while h becomes $= 0$ after the $m-1$ differentiations of $(e^h+1)^{-1}$, which are directed in the first member.

When m is odd (and greater than one), each power, such as $(-e^h)^k$ in the second member, is accompanied by another, namely $(-e^h)^{m-k}$, which is multiplied by the cosine of the same multiple of x; and these two powers destroy each other, when added, if $h = 0$: we arrive therefore in this manner at the known result, that

$$\left(\frac{d}{dh}\right)^{2p}(e^h+1)^{-1} = 0,\ \text{when } h = 0,\ \text{if } p > 0. \ . \ . \ (13.)$$

On the contrary, when m is even, and $h = 0$, the powers $(-e^h)^k$ and $(-e^h)^{m-k}$ are equal, and their sum is double of either; and because

$$\begin{aligned} &(-1)^p\{1 - 2\cos 2x + 2\cos 4x - \cdots \\ &\quad + (-1)^{p-1}\,2\cos(2px-2x)\} = -\frac{\cos(2px-x)}{\cos x}, \end{aligned}$$

by making $m = 2p$ we arrive at this other result, which perhaps is new, that (if $p > 0$ and $h = 0$)

$$\left.\begin{array}{l}\left(\frac{d}{dh}\right)^{2p-1}(e^h+1)^{-1} = \frac{-1.2.3\ldots(2p-1)}{2^{2p-1}\pi} \\ \int_0^\infty dx\left(\frac{\sin x}{x}\right)^{2p}\frac{\cos(2px-x)}{\cos x}.\end{array}\right\} \quad (14.)$$

Developing therefore $(e^h+1)^{-1}$ according to ascending powers of h; subtracting the development from $\frac{1}{2}$, multiplying by h, and changing h to $2h$; we obtain

$$\left.\begin{array}{l}h\frac{e^h-e^{-h}}{e^h+e^{-h}} = \frac{2}{\pi}\int_0^\infty\frac{dx}{\cos x}\Sigma_{(p)1}^{\infty}\left(\frac{h\sin x}{x}\right)^{2p} \\ \cos(2px-x);\end{array}\right\} \quad (15.)$$

that is, effecting the summation, and dividing by h^2,

$$\left.\begin{array}{l}\frac{1}{h}\frac{e^h-e^{-h}}{e^h+e^{-h}} = \frac{2}{\pi}\times \\ \int_0^\infty\frac{dx\,x^{-2}\sin x^2(1-h^2x^{-2}\sin x^2)}{1-2h^2x^{-2}\sin x^2\cos 2x+h^4x^{-4}\sin x^4};\end{array}\right\} (16.)$$

or, integrating both members with respect to h,

$$\left.\begin{array}{l}\int_0^h\frac{dh}{h}\frac{e^h-e^{-h}}{e^h+e^{-h}} = \frac{1}{\pi} \\ \int_0^\infty\frac{dx}{x}\tan x\log\sqrt{\frac{1+hx^{-1}\sin 2x+h^2x^{-2}\sin x^2}{1-hx^{-1}\sin 2x+h^2x^{-2}\sin x^2}}.\end{array}\right\} (17.)$$

It might seem, at first sight, from this equation, that the integral in the first member ought to vanish, when taken from $h=0$ to $h=\infty$; because, if we set about to develope the second member, according to descending powers of h, we see that the coefficient of h^0 vanishes; but when we find that, on the same plan, the coefficient of h^{-1} is infinite, being $=\frac{2}{\pi}\int_0^\infty dx$, we perceive that this mode of development is here inappropriate: and in fact, it is clear that the first member of the formula (17.) increases continually with h, while h increases from 0.

4. Again, since

$$\frac{-h}{e^h+1} = \psi(2h)-\psi(h), \quad \text{if } \psi(h) = \frac{h}{e^h-1}, \quad . \quad . \quad (18.)$$

we shall have (for $p>0$) the expression

$$A_{2p} = \frac{2^{1-2p}\pi^{-1}}{2^{2p}-1}\int_0^\infty dx\left(\frac{\sin x}{x}\right)^{2p}\frac{\cos(2px-x)}{\cos x}, \quad . \quad (19.)$$

if, according to a known form of development, which the foregoing reasonings would suffice to justify, we write

$$\frac{h}{e^h-1}+\frac{h}{2} = 1 + A_2 h^2 + A_4 h^4 + A_6 h^6 + \&c. \quad . \quad (20.)$$

If p be a large number, the rapid and repeated changes of sign of the numerator of the fraction $\frac{\cos(2px-x)}{\cos x}$ produce nearly a mutual destruction of the successive elements of the integral (19.), except in the neighbourhood of those values of x which cause the denominator of the same fraction to vanish; namely those values which are odd positive multiples of $\frac{\pi}{2}$ (the integral itself being not extended so as to include any negative values of x). Making therefore

$$x = (2i-1)\frac{\pi}{2} + \omega, \quad . \quad . \quad . \quad . \quad (21.)$$

in which i is a whole number > 0, and ω is positive or negative, but nearly equal to 0; we shall have

$$\cos(2px-x) = (-1)^{p+i-1}\sin(2p\omega-\omega),$$

exactly, and $\cos x = (-1)^i\omega$, nearly; changing also $\left(\frac{\sin x}{x}\right)^{2p}$ to the value which it has when $\omega = 0$, namely $\left(\frac{2}{\pi}\right)^{2p}(2i-1)^{-2p}$; and observing that

$$\int_{-\omega}^{\omega} d\omega\,\frac{\sin(2p\omega-\omega)}{\omega} = \pi, \text{ nearly}, \quad . \quad . \quad (22.)$$

even though the extreme values of ω may be small, if p be very large; we find that the part of A_{2p}, corresponding to any one value of the number i, is, at least nearly, represented by the expression

$$\frac{(-1)^{p-1}2(2i-1)^{-2p}}{(2^{2p}-1)\pi^{2p}}; \quad . \quad . \quad . \quad (23.)$$

which is now to be summed, with reference to i, from $i = 1$ to $i = \infty$. But this summation gives rigorously the relation

$$\Sigma_{(i)\,1}^{\ \infty}(2i-1)^{-2p} = (1-2^{-2p})\,\Sigma_{(i)\,1}^{\ \infty}i^{-2p}; \quad . \quad (24.)$$

we are conducted, therefore, to the expression

$$A_{2p} = (-1)^{p-1} 2 (2\pi)^{-2p} \Sigma_{(i)1}^{\infty} i^{-2p}, \quad . \quad . \quad (25.)$$

as *at least* approximately true, when the number p is large. But in fact the expression (25.) is *rigorous* for all whole values of p greater than 0; as we shall see by deducing from it an analogous expression for a Bernoullian number, and comparing this with known results.

5. The development

$$\frac{1}{\epsilon^h - 1} + \frac{1}{2} = h^{-1} + B_1 \frac{h}{1.2} - B_3 \frac{h^3}{1.2.3.4} + \&c., \ (26.)$$

being compared with that marked (20.), gives, for the pth Bernoullian number, the known expression

$$B_{2p-1} = (-1)^{p-1} 1.2.3.4 \ldots 2p \, A_{2p}; \quad . \quad . \quad (27.)$$

and therefore, rigorously, by the equation (19.) of the present paper,

$$\left. \begin{array}{l} B_{2p-1} = \dfrac{(-1)^{p-1} 1.2 \ldots 2p}{2^{2p-1}(2^{2p}-1)\pi} \\ \displaystyle\int_0^\infty dx \left(\frac{\sin x}{x}\right)^{2p} \frac{\cos(2px - x)}{\cos x}; \end{array} \right\} \quad . \quad . \quad . \quad (28.)$$

a formula which is believed to be new. Treating the definite integral which it involves by the process just now used, we necessarily obtain the same result as if we combine at once the equations (25.) and (27.). We find, therefore, in this manner, that the equation

$$\frac{2^{2p-1} \pi^{2p} B_{2p-1}}{1.2.3.4 \ldots 2p} = \Sigma_{(i)1}^{\infty} i^{-2p}, \quad . \quad . \quad . \quad (29.)$$

(in which, by the notation here employed,

$$\Sigma_{(i)1}^{\infty} i^{-2p} = 1^{-2p} + 2^{-2p} + 3^{-2p} + \&c.)$$

is *at least nearly true*, when p is a large number; but Euler has shown, in his *Institutiones Calculi Differentialis* (vol. i. cap. v. p. 339. ed. 1787), that this equation (29.) is *rigorous*, each member being the coefficient of u^{2p} in the development of $\frac{1}{2}(1 - \pi u \cot \pi u)$. [See also Professor De Morgan's Treatise on the Diff. and Int. Calc., 'Library of Useful Knowledge,' part xix. p. 581.] The analysis of the present paper is therefore not only verified generally, but also the modifications which were made in the form of that definite integral which entered into our rigorous expressions (19.) and

(28.) for A_{2p} and B_{2p-1}, by the process of the last article, (on the ground that the parts omitted or introduced thereby must at least nearly destroy each other, through what may be called the "principle of fluctuation,") are now seen to have produced no ultimate error at all, their mutual compensation being perfect; a result which may tend to give increased confidence in applying a similar process of approximation, or transformation, to the treatment of other similar integrals; although the logic of this process may deserve to be more closely scrutinized. Some assistance towards such a scrutiny may be derived from the essay on "Fluctuating Functions," which has been published by the present writer in the second part of the nineteenth volume of the Transactions of the Royal Irish Academy.

6. It may be worth while to notice, in conclusion, that the property marked (7.) of the definite integral (1.), enables us to change $\frac{\cos(2px - x)}{\cos x}$ to $\sin 2px \tan x$, in the equations (14.), (15.), (19.), (28.); so that the pth Bernoullian number may rigorously be expressed as follows:—

$$\left.\begin{aligned} B_{2p-1} &= \frac{(-1)^{p-1}.1.2\ldots 2p}{2^{2p-1}(2^{2p}-1)\pi} \\ &\int_0^\infty dx \left(\frac{\sin x}{x}\right)^{2p} \sin 2px \tan x; \end{aligned}\right\} \quad . \quad . \quad . \quad (30.)$$

under which form the preceding deduction of its transformation (29.) admits of being slightly simplified. The same modification of the foregoing expressions conducts easily to the equation

$$\left.\begin{aligned} &\log \frac{e^h + e^{-h}}{2} = \\ &\frac{1}{\pi}\int_0^\infty dx \tan x \tan^{-1} \frac{h^2 \sin x^2 \sin 2x}{x^2 - h^2 \sin x^2 \cos 2x}; \end{aligned}\right\} \quad . \quad (31.)$$

in which $\tan^{-1}$ is a characteristic equivalent to arc tang., and which may be made an expression for log sec x, by merely changing the sign of h^2 in the last denominator; and from this equation (31.) it would be easy to return to an expression for the coefficients in the development of $\frac{e^h - e^{-h}}{e^h + e^{-h}}$, or in that of tan h, and therefore to the numbers of Bernoulli. Those numbers might thus be deduced from the

following very simple equation:

$$\pi \log \sec h = \int_0^\infty d\,x\,y \tan x; \quad . \quad . \quad . \quad (32.)$$

in which y is connected with x and h by the relation

$$\frac{\sin y}{\sin (2\,x - y)} = \left(\frac{h \sin x}{x}\right)^2. \quad . \quad . \quad . \quad (33.)$$

Observatory of Trinity College, Dublin,
October 6, 1843.

XLVI. *Note on the Changes in Colour exhibited by solutions of Chloride of Copper.* *By* E. Solly, *F.R.S.* *In a Letter to* Richard Taylor, *Esq.*

THE colour of the different salts of copper varies, it is well known, according to the proportion of water which they contain; thus the sulphate when anhydrous is perfectly white; whilst in its ordinary crystallized state, when containing five atoms of water, it is of a deep blue colour; most of the other soluble salts of this metal exhibit similar changes in colour according to their state of hydration, none perhaps exhibit them in a more remarkable manner than the chloride. I was recently misled, through ignorance of one of the conditions which modify these changes; and as the colour of solutions of copper has sometimes been supposed to indicate the state in which the metal existed in solutions, I have thought that a brief statement of the appearance of chloride of copper under different circumstances might save others from falling into the same error. Dry chloride of copper is of a yellowish-brown colour; when dissolved in a small quantity of water it forms a dark brown solution; if further diluted it becomes grass-green; and if more water yet be added, the solution becomes bright blue. If, on the other hand, a blue and dilute solution be gradually evaporated, it passes through the various shades of green-blue, blue-green, green and brown. When in place of slowly evaporating a blue and dilute solution it is suddenly heated, it immediately begins to change colour, and when boiling is bright green. This effect appears to be due to heat alone; that it is quite distinct from the change from blue to green, caused by concentrating the solution, is proved by the fact, that if a blue solution be made to boil in a flask it becomes perfectly green whilst boiling, but regains its original blue colour if allowed to cool; this change from blue to green, and *vice versâ*, may be repeated any number of times at pleasure, by alternately heating and cooling the solution, and without concentrating or diluting it.

When solutions of common salt and sulphate or nitrate of copper are mixed together at common temperature, the solution remains blue; on applying heat to it, it gradually changes to green, and if allowed subsequently to cool, regains its original blue colour. It has been supposed from this appearance that common salt does not at common temperature decompose sulphate or nitrate of copper, but that at the boiling temperature it is able to effect their decomposition, and that the green colour of the boiling solution is evidence of the presence of chloride of copper. The facts above stated, however, show that the green colour which the solution acquires when heated is no proof of the formation of chloride of copper, as a dilute solution of the chloride itself is blue when cold, and becomes green when heated. When the mixed solution of nitrate of copper and common salt is evaporated, it becomes permanently green; hence it was supposed that under these circumstances the salts completely decomposed each other, whilst, when the solutions were dilute, the affinities remained so balanced, that a slight increase or decrease in the temperature was sufficient to determine the arrangement of the acids and bases. The truth however evidently is, that the chloride is formed when the cold solutions are mixed, and becomes permanently green from concentration, like the blue dilute solution of the chloride. That this is really the case is also easily shown by taking, in place of solutions of the two salts, the dry sulphate of copper and chloride of sodium in powder; mixing them and adding a small quantity of water, a deep green solution of chloride of copper, together with sulphate of soda, is immediately formed. When sulphate of copper is decomposed by chloride of calcium or barium, a blue or green solution of chloride of copper results, after separating the sulphate of lime or barytes, the colour of which depends on the strength of the solutions originally employed; if weak and blue, it acquires a green colour when heated; and if concentrated, becomes permanently green.

October 15th, 1843.

XLVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 57.]

March 30, 1843. — THE following papers were read, viz.—

1. "Researches into the Structure and Developement of a newly discovered parasitic Animalcule of the Human Skin, the *Entozoon folliculorum*." By Erasmus Wilson, Esq., Lecturer on Anatomy and Physiology at the Middlesex Hospital. Communicated by R. B. Todd, M.D., F.R.S.

While engaged in researches on the minute anatomy of the skin and its subsidiary organs, and particularly on the microscopical composition of the sebaceous substance, the author learned that Dr. Gustow Simon of Berlin had discovered an animalcule which inhabits the hair follicles of the human integument, and of which a description was published in a memoir contained in the first Number of Müller's Archiv for 1842. Of this memoir the author gives a translation at full length. He then states that, after careful search, he at length succeeded in finding the parasitic animals in question, and proceeded to investigate more fully and minutely than Dr. Simon had done the details of their structure, and the circumstances of their origin and developement. They exist in the sebaceous follicles of almost every individual, but are found more especially in those persons who possess a torpid skin; they increase in number during sickness, so as in general to be met with in great abundance after death. In living and healthy persons, from one to three or four of these entozoa are contained in each follicle. They are more numerous in the follicles situated in the depression by the side of the nose; but they are also found in those of the breast and abdomen, and on the back and loins. Their form changes in the progress of their growth. The perfect animal presents an elongated body, divisible into a head, thorax, and abdomen. From the front of the head proceed two moveable arms, apparently formed for prehension: and to the under side of the thorax are attached four pairs of legs, terminated by claws. The author distinguishes two principal varieties of the adult animal; the one remarkable for the great length of the abdomen and roundness of the caudal extremity; whilst the other is characterized by greater compactness of form, a shorter abdomen, and more pointed tail. The first variety was found to measure, in length, from the one-100th to the 45th, and the second, from the one-160th to the 109th part of an inch.

The author gives a minute description of the ova of these entozoa, which he follows in the successive stages of their developement. The paper is accompanied by numerous drawings of the objects described.

2. "On Factorial Expressions, and the Summation of Algebraic Series." By W. Tate, Esq. Communicated by the Rev. Henry Moseley, M.A., F.R.S., &c.

This paper, which is wholly analytical, contains an investigation of certain general methods for the summation of algebraic series, which have led the author to the discovery of some curious and elegant propositions relative to factorials and the decomposition of fractions; and also to a new demonstration of Taylor's theorem.

3. "Notice of the Comet;" in a Letter from Captain John Grover, F.R.S., addressed to P. M. Roget, M.D., Sec. R.S., and dated from Pisa, March 21st, 1843.

The author states that at Pisa, on Friday, the 17th of March, 1843, at eight o'clock in the evening, he saw a luminous arc in the heavens, extending from a spot about a degree to the south of Rigel to some clouds which bounded the western horizon. It was about 40 minutes

in width; the edges sharply and clearly defined. On the 20th of March, the author could distinctly trace the extremity of the luminous streak, which he concluded was the tail of a comet, below the lower part of the constellation Orion, and reaching to the star η Eridani; while the stars δ and ϵ Eridani were distinctly seen with the naked eye through the coma. From η Eridani, it extended 47° 30′ to a spot nearly equidistant from χ Orionis and η Leporis*.

4. "Variation de la Déclinaison et Intensité Horizontales Magnétiques observées à Milan pendant vingt-quatre heures consécutives le 18 et 19 Janvier, et le 20 et 25 Fevrier 1843." Par C. Carlini, For. Mem. R.S.

5. A paper was also in part read, entitled "On the general and minute Structure of the Spleen in Man and other Animals." By William Julian Evans, M.D. Communicated by P. M. Roget, M.D., Sec. R.S.

April 6.—The following papers were read, viz.—

1. "On the general and minute Structure of the Spleen in Man and other Animals." By William Julian Evans, M.D. Communicated by P. M. Roget, M.D., Sec. R.S.

After adverting briefly to the discordant opinions of Malpighi, Ruysch, and others regarding the structure of the spleen, the author proceeds to detail the results of the investigations on this subject, in which he has been for many years engaged. According to his analysis, the following are the component parts of this organ:—first, a reticulated fibro-elastic tissue; secondly, a pulpy parenchyma, containing the Malpighian glands and the splenic corpuscles; thirdly, distinct cellular bodies; fourthly, the usual apparatus of arteries, veins, lymphatics and nerves; fifthly, certain fluids; and lastly, the membranes or tunics by which it is invested.

He describes the cells of the spleen as being formed of a lining membrane, continued from that of the splenic vein, and strengthened by filaments of the fibro-elastic tissue. The splenic vein communicates with these cells, at first by round foramina, then by extensive slits resembling lacerations; and it ultimately loses itself entirely in the cells. The cells themselves communicate freely with one another, and also with the veins of the parenchyma; and may therefore be considered as in some measure continuations of the veins. This structure constitutes a multilocular reservoir of great extensibility, and possessing great elastic contractility; properties, however, which exist in a much less degree in the human spleen than in that of herbivorous animals; in which animals the cellated structure itself is much more conspicuous, and predominates over the parenchymatous portion. As the splenic artery has no immediate communication with the cells, these latter may be filled much more readily by injection from the vein than from the artery. In the ordinary state of the circulation, the blood, which has passed into the cells from the veins, is pressed into the branches of the splenic veins by a force derived from the elasticity of the fibro-elastic tissue which surrounds

[* Other notices of the comet have appeared, in the present volume, pp. 54, 147, 311; and in the preceding volume, p. 323.]

the cavities of the cells, thus constituting a *vis-a-tergo*, which contributes to propel the blood onwards in its circulation through the liver. Should there arise, however, any obstructing cause which the resilience of the spleen is unable to overcome, a regurgitation must take place, leading to a congestion both in the mesenteric and splenic veins. The spleen may thus serve as a receptacle for the blood of the abdominal circulation during any temporary check to its free passage into the vena cava; a purpose which is more fully answered in herbivorous animals in whom the abdominal circulation is more extensive, and the spleen is of larger dimensions and greater elasticity.

The splenic corpuscles are thickly scattered throughout the cellular parenchyma of this organ; and from each corpuscle there arises a minute lymphatic vessel; the interlacing of adjacent lymphatics giving rise to a fine and extensive net-work. The trunks of these vessels enter into the Malpighian glands, and again ramifying, form a lymphatic plexus in the interior of these bodies. The fluid contents of these vessels, which had been before pellucid, is now found to contain white organic globules, similar in every respect to those observed in the fluid of lymphatic glands in other parts of the body. The author considers the secretion of this fluid, which appears to be identical with the contents of the lymphatic glands, as being the peculiar function of the splenic parenchyma.

A few illustrative drawings and diagrams accompany this paper.

2. "On the Structure and Developement of the Nervous and Circulatory Systems, and on the existence of a complete Circulation of the Blood in Vessels in the Myriapoda and the Macrourous Arachnida." By George Newport, Esq. Communicated by P. M. Roget, M.D, Sec. R.S.

This paper is the first of a series which the author proposes to submit to the Royal Society on the comparative anatomy and the developement of the nervous and circulatory systems in articulated animals. Its purpose is, in the first place, to investigate the minute anatomy of the nervous system in the Myriapoda and the Macrourous Arachnida, and more especially with reference to the structure of the nervous cord and its ganglia; and thence to deduce certain conclusions with respect to the physiology of that system and the reflex movements in vertebrated animals; secondly, to demonstrate the existence of a complete system of circulatory vessels in the Myriapoda and Arachnida; and thirdly, to point out the identity of the laws which regulate the developement of the nervous and circulatory systems throughout the whole of the Articulata, and the dependence of these systems on the changes which take place in the muscular and tegumentary structures of the body, as, in a former paper, he showed was the case with regard to the changes occurring in the nervous system of true insects.

The first part of the paper relates to the nervous system. A description is given of this system in the Chilognatha, which the author was led, by his former investigations, to regard as the lowest order of the Myriapoda, and approximating most nearly to the

Annelida. He traces the different forms exhibited by the nervous system in the principal genera of that order, the most perfect of which are connected on the one hand with the Crustacea, and on the other with true insects. Passing from these to the Geophili, the lowest family of the Chilopoda, which still present the vermiform type, the nervous system is traced to the tailed Arachnida, the Scorpions, through Scolopendra, Lithobius and Scutigera; the last of which tribes connects the Myriapoda on the one hand with the true insects, and on the other with the Arachnida. The brain and the visceral nerves, the coverings and structure of the cord and ganglia, and the distribution of the systemic nerves are examined in each genus, but more particularly in the Scorpion, in which the nerves of the limbs are traced to the last joints of the tarsi, and those of the tail to the extremity of the sting. Especial attention is bestowed on the structure of the cord and its ganglia, and their developement during the growth of the animal. In the lowest forms of the Iulidæ, in which the ganglia are very close together, and hardly distinguishable from the non-ganglionic portions of the cord, the author has satisfactorily traced four series of fibres, a superior, and an inferior one, and also a transverse and a lateral series. The superior series, which he formerly described in insects as the motor tract, he has assured himself is distinct from the inferior, which he regarded as the sensitive tract; this evidently appears on examining the upper and under sides of a ganglionic enlargement of the cord. On the upper surface the direction of the fibres is perfectly longitudinal; while the fibres on the under surface are enlarged, and curvilinear in their direction. But he remarks that it is almost impossible to determine by experiment whether these structures are separately motor and sensitive, as formerly supposed, or whether they both administer to these functions by an interchange of fibres. These two series appear also to be separated in each ganglionic enlargement of the cord by the third series, constituting the transverse or commissural fibres, which pass transversely through the ganglia, and of which the existence was first indicated by the author in his paper on the *Sphinx ligustri*, published in the Philosophical Transactions for 1834*. The author states that, in addition to these, there is in each half of the cord another and more important series of fibres, which constitute a large portion of the cord, but of which the existence has hitherto entirely escaped observation. This series forms the lateral portion of each half of the cord, and differs from the superior and inferior series in the circumstance, that while those latter series are traceable along the whole length of the cord to the subœsophageal and cerebral ganglia, the former series extends only from the posterior margin of one ganglion to the anterior margin of the first or second beyond it; thus bounding the posterior side of one nerve and the anterior of another, and forming part of the cord only in the interval between the two nerves. From this circumstance, the author designates the fibres of this series, *fibres of reinforcement of the cord*. Every nerve proceeding from a ganglionic enlargement is composed of these four

[* An abstract of which was given in Phil. Mag. S. 3. vol. vi. p. 55.]

sets of fibres, namely, an upper and an under one, communicating with the cephalic ganglia; a transverse or commissural, which communicates only with corresponding nerves on the opposite side of the body; and a lateral set, which communicates only with nerves from another ganglionic enlargement on the same side of the body, and which forms part of the cord in the interspace between the ganglia. The author had long suspected that this latter set of fibres existed; but he had never, until lately, ascertained their presence by actual observation. Their action seems fully to account for the reflected movements of parts both anterior and posterior to an irritated limb; as that of the commissural set does the movements of parts situated on the opposite side of the body to that which is irritated. In the ganglia of the cord in Iulus and Polydesmus, the fibres of the inferior longitudinal series are enlarged and softened on entering the ganglion, but are again reduced to their original size on leaving it; thus appearing to illustrate the structure of ganglia in general. In the developement of the ganglia and nerves in these genera, and also in Geophilus, the same changes take place as those which were formerly described by the author as occurring in insects; namely, an aggregation of ganglia in certain portions of the cord, and shifting of the position of certain nerves, which at first exist at ganglionic portions of the cord, but afterwards become removed to a non-ganglionic portion. The nervous cord is elongated, in order that it may keep pace with the growth of the body, which is periodically acquiring additional segments: that this elongation takes place in the ganglia is proved by these changes of position in the nerves lying transversely across the ganglia. The author infers from these facts, that the ganglia are centres of growth and nourishment, as well as of reflex movements, and that they are analogous to the enlargements of the cord in the vertebrata.

A series of experiments on the Iulus and Lithobius are next related; the result of which shows that the two supra-œsophageal ganglia are exclusively the centres of volition, and may therefore strictly be regarded as performing the functions of a brain: so that when these ganglia are injured or removed, all the movements of the animal are of a reflex character. When, on the other hand, these ganglia are uninjured, the animal movements are voluntary, and there exists sensibility to pain: there is, however, no positive evidence that the power of sensation does not also reside in the other ganglia.

The second part of the paper relates to the organs of circulation. In all the Myriapoda and Arachnida the dorsal vessel or heart is divided, as in insects, into several compartments, in number corresponding to the abdominal segments. Its anterior portion is divided, immediately behind the basilar segment of the head, into three distinct trunks. The middle portion, which is the continuation of the vessel itself, passes forwards along the œsophagus, and is distributed to the head itself; while the two others, passing laterally outwards and downwards in an arched direction, form a vascular collar round the œsophagus, beneath which they unite in a single vessel, as was

first noticed by Mr. Lord in the Scolopendra. This single median vessel lies above the abdominal nervous cord, and is extended backwards throughout the whole length of the body as far as the terminal ganglia of the cord, under which it is subdivided into separate branches accompanying the terminal nerves to their final distribution. Immediately anterior to each ganglion of the cord, this vessel gives off a pair of vascular trunks; and each of these trunks is divided into four arterial vessels, one of which is given to each of the principal nerves proceeding from the ganglion, and may be traced along with it to a considerable distance. Of these, the vessel situated most posteriorly is again connected with the great median trunk by means of a minute branch, so that the four vessels on each side form, with their trunks, a complete vascular circle above each ganglionic enlargement of the cord. Besides these, which may be regarded as the great arterial trunk and vessels conveying the blood directly from the anterior distribution of the heart to the limbs and inferior surface of the body, the author has also discovered a pair of large arterial vessels in each segment, originating directly from the posterior and inferior surface of each chamber of the heart. These vessels he has named *the systemic arteries*; and in the Scolopendra he has traced them from the great chamber of the heart, which is situated in the penultimate segment of the body, to their ultimate distribution and ramification in the coats of the great hepatic vessels of the alimentary canal.

After the blood has passed from the arteries, it is returned again to the heart in each segment of the body by means of exceedingly delicate transparent vessels, which pass around the sides of the segments and communicate with the valvular openings of each chamber of the heart at its upper surface, where the valvular openings are situated, not only in all the Myriapoda, but also in the Scorpionidæ. In Scorpions, the circulatory system is more complete and important than even in the Myriapoda. The heart, divided as in Myriapods into separate chambers, is lengthened out at its posterior extremity into a long caudal artery, and gives off a pair of systemic arteries from each chamber, precisely as in the Myriapoda. These arteries not only distribute their blood to the viscera, but send their principal divisions to the muscular structures of the inferior and lateral parts of the body, as well as to the pulmonary sacs. At the anterior part of the abdomen, the heart becomes aortic, descends suddenly into the thorax, and immediately behind the brain spreads out into several pairs of large trunks, which are given to the head, and to the organs of locomotion. The posterior of these trunks form a vascular collar around the œsophagus, beneath which they unite, anteriorly, to a strong bony arch in the middle of the thorax, to form the great arterial trunk, or supra-spinal vessel, which conveys the blood to the posterior part of the body, as in the Myriapoda. This vessel passes beneath the transverse bony arch of the thorax, and is slightly attached to it by fibrous tissue, which circumstance probably induced Professor Müller, who observed this structure in 1828, to regard it as a ligament. In its course backwards, along the ner-

vous cord, this vessel is gradually lessened in size, until it arrives at the terminal ganglion of the cord in the tail, where it is divided into two branches, which take the course of the terminal nerves, and these are again subdivided before they arrive at their ultimate distribution. In addition to these parts, the author found a hollow fibrous structure, which closely surrounds the cord and nerves immediately after they have passed beneath the arch of the thorax. From the sides of this structure there pass off backwards two pairs of vessels, that get beneath the peritoneal lining of the abdominal cavity and are distributed on the first pair of branchiæ. A small vessel also passes backwards beneath the cava, and, being joined by anastomoses from the spinal artery, form the commencement of a vessel which the author formerly described in the 'Medical Gazette' as the *subspinal vessel.* This vessel, extending along the under surface of the nervous cord, communicates directly, by short vessels, with the supra-spinal artery, and gives off, at certain distances from its under surface, several large vessels, which unite with others that convey the blood which has circulated through the abdominal segments, directly to the branchiæ, whence it is returned to the heart by many minute vessels that originate from the posterior internal part of each branchia, and, united into single trunks, pass around the sides of the segments to the valvular openings on the dorsal surface of the heart. In the tail of the Scorpion there is a direct vascular communication between the caudal artery and the subspinal vein, which, from the direction of the vessels, induces a belief that there is some peculiarity in the circulation of the blood in this part of the body. Besides these vessels, the author found an arterial trunk that originates from the commencement of the aorta as it descends into the thorax. This vessel passes backwards along the alimentary canal, to which it is distributed, and gives off branches to the liver.

This paper is accompanied by five drawings, illustrating the anatomical facts which are described in it.

May 11.—The following papers were read, viz.—

1. "Variations de la Déclinaison et Intensité Horizontale magnétique observées à Milan pendant vingt-quatre heures consécutives le 22 et 23 Mars, et le 19 et 20 Avril 1843." Par F. Carlini, For. Mem. R.S.

2. "Note regarding the Observations of T. Wharton Jones, Esq., F.R.S., 'On the Blood Corpuscles*.'" By Martin Barry, M.D., F.R.S. L. & E.

The author observes, that the structure of the blood-corpuscles can be accurately learned only by a careful investigation of their mode of origin, and by following them through all their changes in the capillary vessels, and especially in the capillary plexuses and dilatations, where all their stages of transition from the colourless to the red corpuscles may be seen. The filament which forms here and there in the corpuscles of coagulating blood he has shown to other persons, with Microscopes made by Ross and Powell. Dr. Barry denies that he meant certain general remarks in his paper, re-

[* See Phil. Mag. S. 3. vol. xxii. p. 480.]

ferring to more than twenty delineations of corpuscles from various animals, to apply exclusively to those of man.

3. A paper was also in part read, entitled, "Experiments on the Gas Voltaic Battery, with a view of ascertaining the rationale of its action; and on its application to Eudiometry." By William Robert Grove, Esq., M.A., F.R.S., Professor of Experimental Philosophy in the London Institution.

May 18.—1. The reading of Prof. Grove's paper, entitled "Experiments on the Gas Voltaic Battery, with a view of ascertaining the rationale of its Action," &c., was resumed and concluded.

The author, referring to a paper published in the Philosophical Magazine for December 1842 [S. 3. vol. xxi. p. 417], giving an account of a voltaic battery of which the active ingredients are gases, and by which the decomposition of water is effected by means of its composition, describes several variations in the form of the apparatus recorded in that paper. The experiments he has made with this new apparatus, and the details of which occupy the greater part of the present memoir, he conceives establish the conclusion that the phenomena exhibited in the gaseous battery are in strict conformity with Faraday's law of definite electrolysis. They also confirm him in the opinion which he had expressed in his original paper, and which had been controverted by Dr. Schœnbein, in a communication to the Philosophical Magazine for March 1843 [S. 3. vol. xxii. p. 165], as well as by other philosophers, namely, that the oxygen, in that battery, immediately contributes to the production of the voltaic current. Besides employing as the active agents oxygen and hydrogen gases, he extends his experiments to the following combinations: namely,

Oxygen and peroxide of nitrogen;
Oxygen and protoxide of nitrogen;
Oxygen and olefiant gas;
Oxygen and carbonic oxide;
Oxygen and chlorine;
Chlorine and dilute sulphuric acid;
Chlorine and solutions of bromine and iodine in alternate tubes;
Chlorine and hydrogen;
Hydrogen and carbonic oxide;
Chlorine and olefiant gas;
Oxygen and binoxide of nitrogen;
Oxygen and nitrogen, with solution of sulphate of ammonia;
Carbonic acid and carbonic oxide, with oxalic acid as an electrolyte;
Hydrogen, nitrogen, and sulphate of ammonia.

The author concludes, on reviewing the whole of this series of experiments, that, with the exception, perhaps, of olefiant gas, which appears to give rise to an extremely feeble current, chlorine and oxygen, on the one hand, and hydrogen and carbonic oxide, on the other, are the only gases which are decidedly capable of electro-synthetically combining so as to produce a voltaic current. He thinks that the vapours of bromine and of iodine, were they less soluble, would probably also be found efficient as electro-negative gases.

He proceeds to consider, in the remaining part of his paper, the application of the gas battery to the purposes of eudiometry, founded on the circumstance already mentioned, that nitrogen gas, as well as several other gases, are absolutely without effect in as far as regards any alteration of their volume, and may therefore be advantageously employed in the analysis of atmospheric air, or other mixed gases. Several experiments of this nature are described, and others suggested for future trial. Various theoretical views, arising from this train of inquiry, are then discussed; particularly with reference to the contact theory, with which the author conceives that the action of the gas battery is not reconcileable; and also to the source of the caloric evolved during voltaic action, which he is strongly inclined to think is in the battery itself.

2. A paper was also read, entitled "Contributions to Terrestrial Magnetism." No. IV. By Lieut.-Colonel Edward Sabine, R.A., F.R.S.*

In the present number of these contributions, the author resumes the consideration of Captain Sir Edward Belcher's magnetic observations, of which the first portion, namely, that of the stations on the north-west coast of America and its adjacent islands, was discussed in No. 2. The return to England of H.M.S. Sulphur by the route of the Pacific Ocean, and her detention for some months in the China Seas, have enabled Sir Edward Belcher to add magnetic determinations at thirty-two stations to those at the twenty-nine stations previously recorded.

The author first describes the experiments which he instituted with the different needles employed by Captain Sir Edward Belcher for determining the coefficient to be employed in the formula for the temperature corrections; and takes this opportunity of noticing the singular fact that, in needles made of a particular species of Russian steel, this coefficient is negative; that is, in these needles, an increase of temperature increases the magnetic power. M. Adolphe Erman describes this particular kind of steel as consisting of alternate very thin layers of soft iron and of steel, so that when heated the soft iron layers increase their magnetic intensity and the steel layers diminish theirs.

He next considers the more important question of the steadiness with which the needles may have maintained their magnetic condition. By intercomparison of the results obtained at various stations with the different needles employed, he assigns corrections to be applied to the determination of the magnetic force deduced from the observed times of vibration. The magnetic force thus corrected, from the observations with each of the needles employed at the various stations visited by Sir Edward Belcher, is then given in a general table of results. The observations of the inclination of the needle are given in another table; and a third table contains the

[* A notice of No. III. of these Contributions was given in Phil. Mag. S. 3. vol. xx. p. 506; and of No. II. in vol. xviii. p. 549; for No. V. see p. 380.]

determination of the declination and inclination of the needle, the horizontal and total magnetic intensity deduced from the observations at thirty-two stations, of which the latitudes and longitudes are given in the same table, together with the dates of observation.

May 25.—The following papers were read, viz.—

1. "Meteorological Journal, from January to April inclusive, 1843, kept at Guernsey." By Samuel Elliott Hoskins, M.D. Communicated by Samuel Hunter Christie, Esq., Sec. R.S.

2. "On the Respiration of the Leaves of Plants." By William Hasledine Pepys, Esq., F.R.S.

The author gives an account of a series of experiments on the products of the respiration of plants, and more particularly of the leaves; selecting, with this view, specimens of plants which had been previously habituated to respire constantly under an inclosure of glass; and employing, for that purpose, the apparatus which he had formerly used in experimenting on the combustion of the diamond*, and consisting of two mercurial gasometers, with the addition of two hemispheres of glass closely joined together at their bases, so as to form an air-tight globular receptacle for the plant subjected to experiment.

The general conclusions he deduces from his numerous experiments conducted during several years, are, first, that in leaves which are in a state of vigorous health, vegetation is always operating to restore the surrounding atmospheric air to its natural condition, by the absorption of carbonic acid and the disengagement of oxygenous gas: that this action is promoted by the influence of light, but that it continues to be exerted, although more slowly, even in the dark. Secondly, that carbonic acid is never disengaged during the healthy condition of the leaf. Thirdly, that the fluid so abundantly exhaled by plants in their vegetation is pure water, and contains no trace of carbonic acid. Fourthly, that the first portions of carbonic acid gas contained in an artificial atmosphere, are taken up with more avidity by plants than the remaining portions; as if their appetite for that pabulum had diminished by satiety.

3. A paper was also in part read, entitled "On the minute Structure of the Skeletons or hard parts of the Invertebrata." Part II. By William B. Carpenter, M.D. Communicated by the President.

June 1.—The following papers were read, viz.—

1. "Magnetic-term Observations for January, February, March, and April 1843," made at the Observatory at Prague, by Professor Kreil. Communicated by Samuel Hunter Christie, Esq., Sec. R.S.

2. "Hourly Meteorological Observations, taken between the hours of 6 A.M. March 17th, 1843, and 6 A.M. of the following day, being the period of the Spring Tides of the Vernal Equinox, at Georgetown, British Guiana." By Daniel Blair, Esq., the Colonial Surgeon, transmitted by Henry Light, Esq. Communicated by the Lords Commissioners of the Admiralty.

[* The paper in which this apparatus is described was inserted in Phil. Mag. S. 1. vol. xxix. p. 216.]

3. "On the minute structure of the Skeletons, or hard parts of Invertebrata." By W. B. Carpenter, M.D. Communicated by the President. Part II. "On the structure of the Shell in the several families and genera of Mollusca *."

The author here gives in detail the results of his inquiries into the combinations of the component elements of shell as they are met with in the several families and genera of the Mollusca; and considers all these results as tending to establish the general proposition, that where a recognizable diversity presents itself in the elementary structure of the shell, in different groups, that diversity affords characters which indicate the natural affinities of the several genera included in those groups, and which may therefore be employed with advantage in classification, and in the recognition and determination of fossils.

June 15.—The following papers were read, viz.—

1. "On the supposed developement of the Animal Tissues from Cells." By James Stark, M.D., F.R.S.E. Communicated by James F. W. Johnston, Esq., M.A., F.R.S.

The author controverts the prevailing theory of the developement of animal tissues from cells, and denies the accuracy of the microscopical observations on which that theory is founded, as regards the anatomy of the adult as well as of the fœtal tissues. He asserts that at no period of fœtal life can rows of cells be discovered in the act of transformation into muscular fibres: and he denies that these fibres increase either in length or in thickness by the deposition of new cells. He contends that the ultimate filaments of muscles, as well as all the other tissues of the body, are formed from the fibrinous portion of the blood, which is itself composed of globules that are disposed to cohere together, either in a linear series, so as to form a net-work of fine filaments, or in aggregated masses of a form more or less globular, composing what have been termed fibrinous corpuscles. These corpuscles have been considered to be the nuclei of cells; but the author regards them as being merely accidental fragments of broken-down tissues, adhering to the filaments, and noways concerned in their developement. The more regularly disposed granules, which are observed to occupy the spaces intervening between the filaments composing the ordinary cellular tissue, he considers as being fatty matter deposited within these spaces. He, in like manner, regards the observations tending to show the cellular origin of the fibrous, cartilaginous, and osseous tissues, as altogether fallacious; and maintains that the cells, which these animal textures exhibit when viewed under the microscope, are simply spaces occurring in the more solid substance of these structures, like the cavities which exist in bread. These views are pursued by the author in discussing the formation of the skin, the blood-vessels, and the nerves, and in controverting the theory of secretion, founded on the action of the interior surfaces of the membranes constituting cells.

[* For a notice of Part I. of Dr. Carpenter's paper, see our preceding volume, p. 484.]

2. "Contributions to Terrestrial Magnetism."—No. V. By Lieut.-Colonel Edward Sabine, R.A., F.R.S.*

In this paper the author details and discusses the magnetic observations made on board Her Majesty's ships Erebus and Terror, between October 1840 and April 1841, being the first summer which the expedition under the command of Captain James Clark Ross, R.N., passed within the Antarctic Circle.

The elimination of the influence of the ship's iron in the calculation of the results of these observations occupies a considerable portion of the paper. Formulæ for this purpose are derived from M. Poisson's fundamental equations, and the constants in the formulæ are computed for each of the two ships, from observations made on board expressly with that object. With these constants, tables of double entry are formed for each of the three magnetic elements, namely, declination, inclination, and intensity, giving the required corrections of each, for all the localities of the voyage.

These and other corrections being applied, the results are tabulated and charts formed from them. The full consideration of the charts is postponed until the whole of the materials collected by the Antarctic Expedition shall be before the Royal Society. Meanwhile the paper concludes with the following general remarks, viz.

1. The observations of declination, particularly those which point out the course of the lines of 0 and of 10° east, indicate a more westerly position than the one assigned by M. Gauss in the 'Atlas des Erdmagnetismus,' for the spot in which all the lines of declination unite. The progression of the lines in the southern hemisphere generally, from secular change, is from east to west; the difference consequently is in the direction in which a change should be found in comparing earlier with more recent determinations.

2. The general form of the curves of higher inclination in the southern hemisphere is much more analogous to that in the northern than appears in M. Gauss's maps. For example, the isoclinal line of $-85°$, instead of being nearly circular, as represented in the 3te Abtheilung of Plate XVI. of the 'Atlas des Erdmagnetismus,' is an elongated ellipse, much more nearly resembling in form and dimensions the ellipse of 85° of inclination in the northern hemisphere in the same work, Plate XVI. 2te Abtheilung. The analogy between the two hemispheres in the characteristic feature of the elliptical form of the higher isoclinal lines is the more important to notice, on account of the particular relation which appears to subsist in the northern hemisphere between the change in the geographical direction of the greater axis of the ellipse, and the secular changes of the inclination generally throughout the hemisphere. The present direction of the greater axis in the northern hemisphere, is nearly N.N.W. and S.S.E., or that of a great circle passing through the two foci of maximum intensity. In the southern hemisphere, the present direction of the greater axis differs little from E.S.E. and W.N.W.

3. Captain Ross's observations of the intensity do not appear to

[* See *ante*, p. 377.]

indicate the existence anywhere in the southern hemisphere of a higher intensity than would be expressed by 2·1 of the arbitrary scale. In this respect also the analogy between the two hemispheres appears to be closer than is shown in M. Gauss's maps, Plate XVIII. With respect to the direction of as much of the line of highest intensity (2·0) as it has been possible to draw with any degree of confidence from the observations now communicated, it will be found to be in almost exact parallelism with the isodynamic line of 1·7 in Plate III. of the author's report "On the Variations of the Magnetic Intensity," in the Report of the eighth meeting of the British Association, for 1838; which line was the highest of which the position could be assigned at that period for any considerable distance by the aid of the then existing determinations.

3. "An Account of several new Instruments and Processes for determining the Constants of a Voltaic Circuit," by Charles Wheatstone, V.P.R.S., Professor of Experimental Philosophy in King's College, London, Corresponding Member of the Royal Academy of Sciences at Paris, &c.

The author proposes in the present communication to give an account of various instruments and processes which he has employed during several years past for the purpose of investigating the laws of electric currents. He states that the practical object for which these instruments were originally constructed, was to ascertain the most advantageous conditions for the production of electric effects through circuits of great extent, in order to determine the practicability of communicating signals by means of electric currents to more considerable distances than had hitherto been attempted. Their use, however, is not limited to this special object, but extends equally to all inquiries relating to the laws of electric currents and to every practical application of this wonderful agent.

As the instruments and processes described by the author are all founded on Ohm's theory of the voltaic circuit, he commences with a short account of the principal results to which this theory leads, and shows how the clear ideas of electromotive forces and resistances, substituted for the vague notions of intensity and quantity which formerly prevailed, furnish us with satisfactory explanations of phenomena, the laws of which have hitherto been involved in obscurity and doubt. According to Ohm's system, the force of the current is equal to the sum of the electromotive forces divided by the sum of the resistances in the circuit. The several electromotive forces and resistances which enter into the circuit of a voltaic battery are then defined; and having frequent occasion to refer to the laws of the distribution of the electric current in the various parts of a circuit, when a branch conductor is placed so as to divert a portion of the current from a limited extent of that circuit, the author directs particular attention to these laws. After recommending several new terms in order to express general propositions, without circumlocution and with greater precision, the author states the method of obtaining the constants of a circuit employed by Fechner, Lenz, Pouillet, &c., and then proceeds to explain the new method he has

himself adopted. The principle of this method is the employment of variable instead of constant resistances, bringing, thereby, the currents in the circuits compared to equality, and inferring from the amount of the resistance measured out between two deviations of the needle, the electromotive forces and resistances of the circuit according to the particular conditions of the experiment; a method which requires no knowledge of the forces corresponding to different deviations of the needle. To apply this principle, it is requisite to have a means of varying the interposed resistance, so that it may be gradually changed within any required limits. The author describes two instruments for effecting this purpose; one intended for circuits in which the resistance is considerable, the other for circuits in which it is small. The *Rheostat* (for thus the inventor names the instrument under both its forms) may also be usefully employed as a regulator of a voltaic current, in order to maintain for any required length of time precisely the same degree of force, or to change it in any required proportion; its advantages in regulating electro-magnetic engines and in the operations of voltatyping, electro-gilding, &c. are pointed out.

Various methods of measuring the separate resistances in the circuit, particularly that of the rheomoter itself, are next described; and it is shown that the number of turns of the rheostat requisite to reduce the needle of a galvanometer from one given degree to another, is an accurate measure of the electromotive force of the circuit. It is then proved that similar voltaic elements of various magnitudes, conformably to theory, have the same electromotive force; that the electromotive force increases exactly in the same proportion as the number of similar elements arranged in series; and that when an apparatus for decomposing water is placed in a circuit, an electromotive force, opposed to that of the battery, is called into action, which is constant in its amount, whatever may be the number of elements of which the battery consists. The electromotive forces of voltaic elements formed of an amalgam of potassium with zinc, copper and platina, a solution of a salt of the negative metal being the interposed liquid, are given; the last combination is one of great electromotive energy, and when a voltameter is interposed in the circuit, it decomposes abundantly the water contained in it. A still more energetic electromotive force is exhibited by a voltaic element, consisting of amalgam of potassium, sulphuric acid, and peroxide of lead. The author then shows, that if three metals be taken in their electromotive order, the electromotive force of a voltaic combination formed of the two extreme metals is equal to the sum of the electromotive forces of the two elements formed of the adjacent metals.

Among the instruments and processes described in the subsequent part of the memoir are the following. 1. An instrument for measuring the resistance of liquids, by which the errors in all previous experiments are eliminated, particularly those resulting from neglecting the contrary electromotive force arising from the decomposition of the liquid. 2. The differential resistance measurer, by means of which the resistances of bodies may be measured in the most accu-

rate manner, however the current employed may vary in its energy. 3. An instrument for ascertaining readily what degree of the galvanometric scale corresponds to half the intensity indicated by any other given degree. 4. A means of employing the same delicate galvanometer to measure currents of every degree of energy, and in all kinds of circuits. 5. Processes to determine the deviations of the needle of a galvanometer corresponding to the degrees of force, and the converse.

4. "On the Organ of Hearing in Crustacea." By Arthur Farre, M.D., F.R.S.

The author finds that in the Lobster (*Astacus marinus*), the organ of hearing consists of a transparent and delicate vestibular sac, which is contained in the base, or first joint of the small antennæ; its situation being indicated externally by a slight dilatation of the joint at this part, and also by the presence of a membrane covering an oval aperture, which is the fenestra ovalis. The inner surface of the sac gives origin to a number of hollow processes, which are covered with minute hairs and filled with granular matter, apparently nervous. A delicate plexus of nerves, formed by the acoustic nerve, which is a separate branch supplied from the supra-œsophageal ganglion, is distributed over the base of these processes and around the sac. Within the sac there are always found a number of particles of siliceous sand, which are admitted, together with a portion of the surrounding water, through a valvular orifice at the mouth of the sac, being there placed apparently for the express purpose of regulating the size of the grains. The author considers these siliceous particles as performing the office of otolites, in the same way as the stones taken into the stomachs of granivorous birds supply the office of gastric teeth. Several modifications of this structure exhibited in the organs of hearing of the *Astacus fluviatilis*, *Pagurus streblonyx*, and *Palinurus quadricornis* are next described, and an explanation attempted of the uses of the several parts and their subserviency to the purposes of that sense.

The author concludes by a description of another organ situated at the base of the large antennæ, which it appears has been confounded with the former by some anatomists, but which the author conjectures may possibly constitute an organ of smell. The paper is accompanied by illustrative drawings.

5. "A statement of Experiments showing that Carbon and Nitrogen are compound bodies, and are made by Plants during their growth." By Robert Rigg, Esq., F.R.S.

The author, finding that sprigs of succulent plants, such as mint, placed in a bottle containing perfectly pure water, and having no communication with the atmosphere except through the medium of water, or mercury and water, in a few weeks grow to more than double their size, with a proportionate increase of weight of all the chemical elements which enter into their composition, is thence disposed to infer that all plants make carbon and nitrogen; and that the quantity made by any plant varies with the circumstances in which it is placed.

6. "Physiological inferences derived from Human and Comparative Anatomy respecting the Origins of the Nerves, the Cerebellum, and the Striated Bodies." By Joseph Swan, Esq. Communicated by Richard Owen, Esq., F.R.S.

The author remarks that those parts of the nervous system which are concerned in motion and in sensation exhibit a great similarity in all vertebrate animals. To the first of these functions belong the anterior and middle portions of the spinal cord and medulla oblongata, including the anterior pyramids, the crura cerebri, and some fibres leading to the corpora striata and the convolutions, and also the cerebellum. To the function of sensation belong the posterior surface of the spinal cord, the posterior and lateral portions of the medulla oblongata, including the posterior pyramids, the ventricular cords, and the fourth and third ventricles.

From a general comparison of the relative magnitude and structure of these several parts in the different classes of vertebrated animals, the author infers that only a very small portion of the brain is necessary for the origins of the nerves, their respective faculties being generally derived near the place at which they leave the brain. These origins are traced in various cases, where, from peculiarities of arrangement or of destination, they present certain remarkable differences of situation.

The author is led to consider the cerebellum as an appendage to the brain, rather than to the medulla oblongata and spinal nerves, for it does not correspond with either the number or the size of the sensitive or motor nerves; and that it is not required for the intellect, for the special senses, for common sensation, or for volition, appears from its size bearing no proportion to the strength of any of these faculties. Neither is it concerned in digestion or assimilation, nor does its size present any relation with the heart, the lungs, the muscles, the limbs, the vertebræ, the ribs, or any other organ, not even those of reproduction. As, however, its nervous connexions are principally with those parts which are exclusively subservient to the will, it is probable that it is concerned in the completion, and not in the commencement of the voluntary act. It is probable, also, that the principal crossing of impulses from one side to the other takes place in the medulla oblongata and the motor tracts of the brain. Some of the arrangements of its lobules may have reference to the paces and attitudes of different animals. The will, acting through the cerebral convolutions, sets in action certain muscles placed in proper directions; but the influence of the cerebellum is required for giving them steadiness amidst the alternations from one set to another, and especially when a slight change disturbs the centre of gravity, and until the balance is effectually restored by a subsequent act of the will operating on antagonist or other muscles. The cerebellum also constitutes an additional focus of nervous influence, and may, therefore, cooperate with the brain in increasing the vital powers, and imparting greater energy to the various functions of the body.

The author regards the corpus striatum as being a centre for con-

veying to the mind the perception of the motions of the limbs and of their different parts. He concludes with some remarks on the double crossings of the tracts of the centres of the nerves of the arms and legs, and the explanation given by these facts to various pathological phenomena.

7. "Nouveaux faits à ajouter à la Théorie de la Chaleur et à celle de l'Évaporation." Par Daniel Parat, Médecin à Grenoble. Communicated by the President.

The author commences by explaining his conception of the nature of heat, of which he gives the following definition:—"Mouvemens centraux obsculaires de la cohésion devenus extemporanément plus rapides, et dilatant de plus en plus tous les corps par une augmentation ainsi acquise de toutes les forces centrifuges." He adopts the theory that the evaporation of water in contact with air is a process identical with chemical solution, and adduces as evidence supporting his views various circumstances which are common both to evaporation and to the solution of a salt in water.

8. "On the nature and properties of Iodide of Potassium, and its general applicability to the cure of Chronic Diseases." By James Heygate, M.D., F.R.S.

The author has been led by his experience to estimate highly the medicinal properties of the iodide of potassium (which he prefers to the tincture of iodine) in various diseases, and thinks that when it is administered judiciously no deleterious effects are likely to arise from its use.

9. "Observations on the relation which exists between the Respiratory Organs of Animals, and the preservation of independent Temperatures." By George Macilwain, Esq., Consulting Surgeon to the Finsbury Dispensary. Communicated by W. Lawrence, Esq., F.R.S.

The author expresses his dissent from the prevailing opinion that the temperature maintained by animals above the surrounding medium is proportionate to the extent of their respiration; and adduces many instances among different classes of animals in which he can trace no such correspondence, and others, on the contrary, where increased powers of respiration appear to diminish instead of raising the animal temperature. Hence the author is disposed to regard respiration as a refrigerating rather than a heating process.

CHEMICAL SOCIETY.

(Continued from p. 77.)

April 18, 1843.—The following papers were read:—

78. "On the Spontaneous Change of Fats," by W. Beetz *.

79. "On certain Improvements in the Instrument, invented by the late Dr. Wollaston, for ascertaining the Refracting Indices of Bodies," by John Thomas Cooper, Esq.

May 2.—The following communications were read:—

* This and all other papers read before the Chemical Society, of which abstracts do not appear in these Proceedings, will be inserted at length in future Numbers of the Philosophical Magazine.

80. "Some additional Remarks on Theine," by J. Stenhouse, Ph.D.

81. "Note on the Preparation of Æther," by George Fownes, Ph.D.

The beautiful experiments of Mitscherlich on the indefinite conversion of alcohol into æther by the same quantity of sulphuric acid, seem to point out the possibility of effecting a great improvement in the economical production of that important substance. It is well known that in the old process, in which equal weights of acid and spirit are subjected to distillation, a large quantity of alcohol escapes ætherification at the commencement of the process, owing to the low boiling-point of the mixture, and on the other hand, much is destroyed towards the end of the distillation by the excessive heat; the limits of temperature within which æther is generated in quantity being, as is well known, rather narrow, ranging perhaps between 280° and 320°.

In the continuous operation described by Mitscherlich such a mixture of alcohol and sulphuric acid is made that its boiling-point shall be well within the æther-producing limit, while into this mixture, maintained in a state of rapid ebullition, alcohol is suffered to flow in such proportion as exactly to replace the liquid which distils over, and which liquid is seen to consist of a mechanical mixture of æther and water with a very small quantity of unaltered alcohol. So long as the temperature is properly maintained by due regulation of the fire and the flow of alcohol, the distilled products do not vary, and the process itself may be, it is said, continued until the oil of vitriol becomes gradually destroyed by the impurities of the spirit, or lost by volatilization.

In this experiment absolute alcohol is used; in the practical manufacture of æther, however, this is obviously impossible; it occurred to me therefore to try experimentally how far the process might be carried if ordinary rectified spirit were substituted. It is stated indeed by Liebig, that under such circumstances ætherification is put a stop to by the accumulation of the water, introduced with the alcohol, gradually depressing the boiling-point of the mixture below the temperature at which æther is formed, and that this happens when the whole quantity of spirit used amounts to four times the weight of the oil of vitriol (*Annalen der Pharmacie*, xxx. 136). It is difficult to see how this could happen if attention were paid to the temperature of the boiling liquid, since it would seem easy to regulate the point of ebullition so as always to maintain the acid of the same degree of concentration with respect to water.

A mixture was made of 6 oz. by weight of concentrated sulphuric acid and $3\frac{3}{4}$ oz. by weight of rectified spirit of sp. gr. ·836 at 60°. This mixture was introduced into a wide-necked flask fitted with a cork pierced with three holes for the purpose of receiving a thermometer, a narrow tube connected with a reservoir of alcohol of the same density as that mentioned above, and a wide tube for conveying the vapours to the condenser, which was a common metal worm immersed in cold water. These arrangements being completed, an Argand gas-lamp was placed beneath the flask, and the contents made to boil; the thermometer speedily rose to near 300° F. A slender stream of spirit was now allowed to mix with the boiling

liquid in such quantity as to maintain at once an invariable temperature and rapid and violent ebullition. It was soon found that by a little management the thermometer could be kept quite stationary at any required point within the limits before referred to. At 300°, and thence to 360°, the separation of the distilled products into two strata was very distinct and beautiful; at 280° to 290°, enough alcohol passed over unchanged to prevent this separation until a little water had been added. There was a slight trace of sulphurous acid, and the mixture in the flask gradually deepened in colour, until at last it became nearly black, without however in the slightest degree losing its efficiency.

At this period the process had been kept up about fifteen hours; more than a gallon of alcohol—twenty times the weight of the acid—had passed through the apparatus, and as the activity of the operation remained to the last unimpaired, it seems fair to infer that its only limits are the loss of sulphuric acid by volatilization, and the formation, in small quantities, of secondary products, such as oil of wine, sulphurous acid, and olefiant gas.

The æther obtained was mixed with some caustic potash and rectified by the heat of warm water; its sp. gr. at 60° was ·730, and it measured fully three pints. As merely water at 55° was used for condensation in place of ice, much loss of vapour must have occurred; and since the residual alkaline liquid yielded a large quantity of alcohol by distillation, the process must be considered on the whole a tolerably productive one, although still very far from what might be desired. Of course, on a large scale much of this loss would be avoided.

It was remarked that during the whole of the operation, even when the temperature was purposely kept so low as to allow much alcohol to escape decomposition, a considerable quantity of permanent gas made its appearance. By adapting to the lower end of the worm of the condenser a two-necked receiver furnished with a bent tube dipping under water, it was easy to collect and examine this gaseous matter. When purified from æther-vapour by washing with oil of vitriol, it was inflammable, burned with much light, and possessed the peculiar alliaceous odour characteristic of purified olefiant gas. Its production became much increased by a rise of temperature; at 310° it passed in a rapid succession of large bubbles.

There appears no difficulty then in applying Mitscherlich's continuous process to the economical manufacture of æther on the great scale; it is very probable too, that by avoiding the use of a naked fire much of the secondary action to which allusion has been made might be prevented, while by a proper condensing arrangement the waste obvious in my own experiments would be avoided. The most advantageous temperature could be determined by experience in a very short time, and with this knowledge the process might be conducted ever afterwards in such a manner as to yield a perfectly uniform product. A somewhat low temperature, about 280° to 290°, might possibly be the most advantageous, since it would be better to let a little alcohol escape ætherification, than to use heat enough

to occasion the abundant production of oil of wine and olefiant gas. This alcohol is easily recovered after the rectification of the æther. It may be proper to mention also that the mixture in the distillatory vessel may be repeatedly suffered to cool, and again reheated without injury.

May 16. The following papers were then read:—

"On the Heat of Chlorine, Bromine and Iodine, developed during the formation of the Metallic Compounds," by Thomas Andrews, M.D., from the Author.

82. "On Ferric Acid," by J. Denham Smith, Esq.

83. "On the Action of Alkalies on Wax," by R. Warington and Wm. Francis, Esqrs.

84. "On the Action of Sulphuric Acid on the Ferrocyanide of Potassium," by George Fownes, Ph.D.

XLVIII. *Intelligence and Miscellaneous Articles.*

ON A CHANGE PRODUCED BY EXPOSURE TO THE BEAMS OF THE SUN, IN THE PROPERTIES OF AN ELEMENTARY SUBSTANCE. BY PROF. DRAPER, OF NEW YORK.

At the recent meeting at Cork of the British Association, a paper by Prof. Draper was read by Dr. Kane before Section A., Mathematical and Physical Science, of which the following is an abstract:—

Prof. Draper's paper commenced with announcing that chlorine gas which has been exposed to the daylight or to sunshine possesses qualities which are not possessed by chlorine made and kept in the dark. It acquires from that exposure the property of speedily uniting with hydrogen gas. This new property of the chlorine arises from its having absorbed tithonic rays, corresponding in refrangibility to the indigo. The property thus acquired is not transient, like heat, but permanent. A certain portion of the tithonic rays is absorbed and becomes latent before any visible effect ensues. Light, in producing a chemical effect, undergoes a change as well as the substance on which it acts; it becomes detithonized. The chemical force of the indigo ray is to that of the red as 66·6 to 1. The author remarked, that we are still imperfectly acquainted with the constitution of elementary bodies, inasmuch as we know in general only those properties which they possess after having been subjected to the influence of light.

ACCOUNT OF CLEGG'S DIFFERENTIAL DRY GAS-LIGHT METER. BY PROFESSOR VIGNOLES, C.E.

To those familiar with gas operations in general, there is no occasion to enlarge on the advantages of a good dry meter. It has been a desideratum ever since the use of a meter at all was first duly appreciated, and has often attracted the attention of many scientific and practical men, who have attempted to realize this desirable object.

This will doubtless be deemed quite a sufficient justification for

so experienced an engineer as Mr. Clegg to present himself before the public as the inventor of an instrument which he considers likely to realize what appears to be so much required.

The construction and action of this meter are based upon the established facts, that the heat and light from the various kinds of carburetted hydrogen gas are strictly proportionate to each other; and, by the application of that fact, in combination with an apparatus, acting on the same principle as the differential thermometer of Leslie.

By this apparatus will be measured most delicately the smallest differences of heat, and consequently the consumption of gas will be registered in proportion to its illuminating power.

Let two hollow glass cylinders, each about one inch in diameter and three inches long, be connected together in the centre of their lengths by a hollow bent tube of the same material (being such as will be afterwards described when treating of the mechanical arrangements for a six-light meter, and delineated in the accompanying figures). Let these cylinders and the connecting tube be perfectly exhausted of air, and let as much alcohol be introduced as will nearly fill one cylinder, leaving a vacuum in the other—or at least leaving it without air, and with only such vapour therein as may arise from the alcohol. Now as pure alcohol boils, *in vacuo*, at 56° of Fahrenheit's thermometer, the smallest excess of heat above this temperature applied to the cylinder having the alcohol therein will cause the liquid to evaporate, and, by its consequent elasticity, will drive the spirit below the vapour into the colder cylinder; and the velocity with which the alcohol will be driven out from one cylinder to the other will be in exact proportion to the quantity of heat applied, twice or three times the cause producing twice or three times the effect, and so on. For, let air or gas be heated to a uniform temperature (say to 150° of Fahrenheit), and, when so heated, let it be directed to impinge upon one of two glass cylinders (such as those above described) with any given velocity; if this velocity be doubled, then double the quantity (or volume or body) of heat will be passed, and consequently double the effect (that is double the velocity with which the alcohol is driven) must be produced, such being an unerring and natural law. Although twice the effect be thus produced, the temperature of the air or gas has not been increased; it is only the flow or quantity which has been augmented; and this must be what is to be understood by quantity of heat. The best criterion of the soundness of the above statement is, that these facts have been determined from, and are founded upon, repeated and concurring experiments—the only true source of philosophical induction.

Such, then, being established, it became a leading principle; and the next step was to ascertain, by further experiments, how to apply this scientific fact to the art of measuring the quantity of heat applied to one of the above-described glass cylinders,and of registering the same; for this being accomplished, there was at once obtained an apparatus, whereby may be determined the exact flow of air or gas in a given time; in other words, a gas-light meter, such as the present instrument. The first consideration therefore was, how to heat a given quantity of gas to a certain uniform temperature, for the gas

being thus heated, and allowed to flow out at a given velocity, a uniform flow of heat was obtained.

Now, for the purposes of measuring this flow of heat, in the instance of a gas-meter, the source of heat is present by the inflammable gas itself; and, after numerous experiments, it was fully and conclusively ascertained that a jet of gas, issuing out of an orifice, perforated in the side of a small solid brass cylinder (such as will be afterwards described, and shown in the annexed figures), will heat the said cylinder to a uniform given temperature, whatever be the height of the said jet: for, with a small flame, the jet clings, as it were, and is in immediate contact with the solid cylinder; whereas, when the flame issues from the orifice with considerable velocity, still the longer jet only imparts the same degree of heat to the solid cylinder as did the smaller one; the increased (or lengthened) flame being, in this latter case, driven away from any closer contact by its own velocity, or rather by the velocity due to the pressure of the gas issuing from the orifice. This fact having been thoroughly established by repeated experiments and practice, the necessary apparatus of the gas-meter, for the practical application of the fact, became very simple.

The next point expedient to be determined accurately, was the proper superficies of a receptacle, to be heated by or from such a solid cylinder as the one just described; which surface would be sufficient to communicate the requisite heat to such portion of the whole quantity of gas to be measured, as it was necessary to pass through the receptacle, without altering the temperature thereof in any perceptible degree. This point was ascertained, as the preceding ones were, by repeated experiments; and, further, it was found advisable that the receptacle for heating the gas should be well clothed with the best non-conducting substance, to keep it at a proper temperature. The proportionate surface of the receptacle having been determined, certain other proportions and dimensions were established, which, as applicable to a six-light meter, are given in the figures accompanying the subsequent mechanical description.

Lastly, it remained to be found what quantity of gas, heated in the manner previously described, and discharged upon one of two such glass cylinders as before mentioned, would be sufficient to expel the alcohol therefrom, and drive it into the other cylinder. Numerous experiments and long practice have determined this quantity to be no more than about one-seventh part of the whole gas requisite to supply the number of burners, the consumption whereof is to be measured.

This being conclusively established, led to the consequent arrangement of dividing the flow of gas as applied from the main, so as to pass them separately through two openings, one of which should have its area six times that of the other, whereby six-sevenths of the gas admitted from the main might flow towards the burners without passing through the working part of the meter, leaving the remaining, or one seventh, part to perform the necessary functions of registering the amount of the whole quantity. The simple manner in which this arrangement is carried into effect, is duly pointed out by the figures, and is described in the subsequent explanation of the whole of the mechanical contrivances for the proper working of the meter.

Two openings, exposed to the stream of the same volume of air or gas, however they may differ in their respective areas, will always admit quantities thereof strictly proportionate to those areas. Thus, through a circular aperture of one inch in diameter, will pass precisely sixty-four times as much air or gas as through a circular aperture of only one-eighth of an inch in diameter; and this will hold unerringly, and under all pressures, allowing only for the additional friction the gas is exposed to in passing through the smaller opening, compared to its friction through the larger one. This theory is so well authenticated by practice, that it can require no further illustration here. It is only the smaller quantity, therefore, or one-seventh part, of the gas which it is necessary actually to pass through the working and registering parts of the meter; and from this portion being very dry and at a high temperature, an immense advantage is derived; for, as the decomposing action on the materials of the meter ceases when the gas is hot and dry, there will be little or none of that wear and tear, going on so rapidly in the ordinary water meter, from the ammonia, sulphur and galvanic action, which are the principal agents of deterioration, and which also act (though not to the same extent) upon other dry meters, only exposed to the usual aqueous vapour which gas absorbs at the ordinary temperature of the atmosphere. In hot dry gas the galvanic action ceases; the ammonia, which exists in the form of a gas, will, when not exposed to aqueous vapour, pass off harmless; but where there is moisture present, the ammoniacal gas is instantly absorbed, and becomes a strong alkaline solution, acting on the wrought iron parts of the meter.

From Sir H. Davy's early experiments, it appears that (taking weight) 100 grains of water absorbs thirty-four grains of ammoniacal gas, consequently (taking bulk) one cubic inch of water takes up 475 cubic inches of that gas. [See Henry's Chemistry, vol. i. p. 397, third edition.) The sulphur in the gas, combining with hydrogen, forms sulphuretted hydrogen gas. Water absorbs twice its own bulk of this gas (see the same work], and when so impregnated, the gas is very destructive to brass and copper; but, when dry, it is harmless.

It will be observed that, in the dry meter now describing, all those parts which are exposed to six-sevenths of the whole quantity of gas admitted, consist either of cast iron, pure tin, or German silver, none of which materials are acted on injuriously by gas in the usual state of the atmosphere; and as there is no water, and the remaining (only one-seventh) part of the gas working through the meter at a high temperature, the action of decomposition on the materials is altogether avoided.

All these requisites having been most conclusively determined *à priori*, it may be proceeded to consider the arrangements necessary to put the above philosophical facts to practical application in the construction of the "New Differential Dry Gas-light Meter."

First, the two glass cylinders, as previously described, are to be suspended in such a manner that the alcohol may be expelled from the one cylinder placed in the lowest position, and driven to occupy the other cylinder placed in a higher position; this being effected, the upper cylinder will descend and act as a pendulum, imparting

motion with a power equal to the weight and height of the fluid raised. To ensure, however, the proper pendulous motion, it is necessary to attach a counterbalance to the weight of the glass cylinders and their connecting tube; this is further required, for the purpose of regulating the quantity of alcohol to be driven into the upper cylinder. The descent of this cylinder constitutes one vibration, the counter weight giving it such sufficient preponderance or momentum in its descent, as to cause it to impart motion, with certainty, to a train of wheel-work, revolving in the way usual in gas-meters; and the vibration, and consequently the corresponding consumption of gas (or light), thereby becomes registered. The manner of suspending the glass cylinders, and fixing the counterbalance weight, is hereafter described.

The next considerations are—1st, how is the tube or receptacle before mentioned (and which will be called the "heater") to be placed over the lower cylinder, so that, in discharging thereupon the hot gas, it may not communicate any of its heat to the upper one? and, 2nd, how to place the upper cylinder in a medium always at the same temperature as that of the room in which the meter is to work? it being absolutely necessary to keep the two cylinders at two greatly opposite temperatures, that shall always bear the same relative degree or difference to each other.

The first of these objects is arrived at easily by placing a tin plate between the two cylinders, as is clearly shown in the figures, and pointed out in the mechanical description following. The second necessary effect is attained by inclosing the whole meter in a thick cast-iron case; in the interior of which, and forming parts of the same casting, two semi-cylindrical projections, or hoods are so placed as nearly to surround that glass cylinder which alternately becomes the uppermost one. The conducting power of this mass of iron is amply sufficient to carry off all the heat radiating from the case of the heater (this heater being, as before stated, enveloped in a case or clothing of the best non-conducting material); and not only so to radiate this heat, but to be always of the same temperature as the room in which the meter is placed. These hoods, therefore, constitute a very essential part of the apparatus; for if the temperature of the upper glass cylinder were to vary materially, so would that of the lower one, and consequently the rate of the meter would also vary; but, by this very simple contrivance, the temperature of the heater, and that of the lower glass cylinder, will be always of the same relative temperature to that of the upper cylinder.

Take an example:—Let the "heater" be at 150° of Fahrenheit, and the room—and therefore the cast-iron parts of the meter—at 60°, then the gas which flows from the heater on to the lower glass cylinder will be at the same temperature, viz. at 150°, and thus the moving power (originating from the meter jet) will be equal to 90°, which represents the heat imparted by the meter jet, such being a constant and uniform quantity. If the temperature of the apartment, and consequently that of the iron case, hoods, &c., be raised to 80°, the temperature of the heater will be increased 20°, becoming 170°, the moving power being constantly 90°. It has been deemed neces-

sary to dwell particularly on this part of the arrangement, which is absolutely essential to the correct registration of the meter, and which has been contrived in so complete and effective a manner, and by means which cannot possibly be deranged.

The preceding are the leading features of this philosophical arrangement for measuring the flow of gas of a given quality. With the same heat the same results will obtain; the only variation that can take place must be by change of temperature of the small jet of flame which issues from the hemispherical end of the solid brass cylinder, this being the governing principle; consequently, with an increased temperature of the solid brass knob, caused by a brighter flame (and which imparts its heat to the tube or receptacle for the gas, called the heater), or *vice versâ* (the same quantity of heated gas being discharged on the lower glass cylinder), the flow of alcohol from the one cylinder to the other, and consequently the vibrations will be quicker or slower in exact proportion to the difference of temperature. Herehy is obtained a measure of light; in fact, a photometer, that is, a light-meter; in other words, a gas-light meter, rather than gas-meter, which is the more accurate definition, since the article to be measured is light, not gas; for it is well known that the illuminating power of coal gas varies 30 per cent.,according to the process used in its production, and the quality of the coal from which the gas is obtained.

The principle of this meter being based on the fact, that the intensity of the heat from a gas flame is as the brightness or illuminating power, it may be well, for the information of those who have not made this branch of chemistry their study, to give the following short extracts from the most approved authorities :—

From Dr. Henry's Experiments on Coal Gas, published in the Manchester Philosophical Transactions :—

"By the first train of experiments, I endeavoured to derive, from a careful analysis of the compound combustible gases, a measure of their illuminating power, admitting of more exact appreciation than the optical method of a comparison of shadows. The one which I was led to propose as the most accurate, and, I still think, entitled to preference, was the determination of the quantities of oxygen gas consumed, and of carbonic acid formed by the combustion of equal measures of the different inflammable gases, that gas having the greatest illuminating power which in a given volume consumes the largest quantity of oxygen. The average results of a great variety of experiments were comprised in the following table :—

Kinds of gas.	Oxygen gas required to saturate 100 measures.	Carbonic acid produced.
Pure hydrogen	50	—
Gas from moist charcoal	60	35
Ditto from wood (oak)	54	33
Ditto from dried peat	68	43
Ditto from cannel coal	170	100
Ditto from lamp oil	190	124
Ditto from wax	220	137
Olefiant	284	179."

From the preceding, it is clearly proved that the illuminating power of gas depends upon the quantity of oxygen consumed, and of carbonic acid produced during combustion.

The object of Dr. Henry's paper was only to prove the illuminating power of the gas; in order therefore to prove that the heat from the combustion of any gas increases in the direct ratio of the oxygen consumed, the following is extracted from Professor Graham's Treatise on Chemistry:—

"From the late researches of Despretz and of Bull, a very interesting rule has been obtained; it is as follows:—'That in all cases of combustion the quantity of heat evolved is proportional to the quantity of oxygen which enters into combination.'"

And in Henry's Chemistry, ninth edition, p. 422, we find it stated, that "by the combustion of denser gases, a higher temperature is produced." See also Williams on Combustion.

The heat and light from the gas having been thus demonstrated to be proportionate to each other, we have, in the preceding apparatus, a meter which will measure the quantity of light given forth, which is the real end to be desired, and the following description of the mechanical arrangements may be now proceeded with.

Figures 1, 2, 3 represent different parts and positions of "Clegg's Patent Differential Dry Gas-light Meter." They are drawn to a scale of 3-10ths of an inch to one inch, or in that proportion to a full-sized meter, capable of measuring the consumption by six large gas-light burners, that is a "six-light meter." The same letters refer to the same parts in all the figures.

A A A is a cylindrical cast iron vessel, being the case of the meter of about a quarter of an inch thickness of metal (made thicker at particular parts requiring it), and five inches outside diameter. P P are hoods or projections, parts of the same casting of the entire case or vessel, the important functions of which have been previously explained. The references to the working parts, distinguished by the other letters, will be made in the subsequent explanations.

For the sake of distinctness and simplicity of description, it may be best to show, first, the manner in which the gas flows through the vessel, and the action of all those parts which are essential to the mere meter, taking up afterwards certain auxiliary mechanical contrivances, quite independent of the general principles on which the meter is constructed, or of the actual operation of registering the light, these contrivances being chiefly introduced for preventing frauds.

When the gas is first turned on at the main stop-cock, it flows into the meter through the openings or pipes, M, to the valve O, which opens into a vertical passage at the back of the meter. Along the upper part, H, of this passage flows a small portion only (viz. one-seventh of the whole), gas finding its way into a receptacle called the "heater," into which it enters through the pipe at I, and thence fills every part of the main body of the meter, where the working parts are disposed; the direction of this flow or current of gas is indicated by short light arrows along its entire course. The remaining

larger portion (viz. six-sevenths) of the gas flows down the lower part, J, of the above vertical passage at the back, and along the horizontal tube, N, at the bottom of the meter, and through the regulating disc or opening, Z, fixed therein, and thence passes direct towards the burners, through the main exit K. The area of this disc, Z, is exactly six times that of the opening, I, into the heater and working parts of the apparatus. This larger portion of the gas flows through these lower passages of the meter without influencing any of the working parts, and for that reason is called the "Neutral Gas," and the direction of its flow or current is marked by long dark arrows. The smaller portion of the whole supply may be called the "Working Gas." A minute quantity of this gas flows from the body of the meter through the small openings, *b b*, perforated in a solid brass cylinder, G, entering at the bottom, where this cylinder is screwed on to the top part of the heater F, and issuing out in the front of the cylinder near its top, in a jet at *c*; this is called the "meter jet," and forms an important part of the arrangement; it may, indeed, be designated as the originator of the moving power; in fact, the prime mover of the meter. The orifice, whence issues the meter jet at *c*, is plugged with platina to prevent corrosion, or any other wear and tear. This meter jet, immediately after opening the main stop-cock, should be lighted, that it may at once impart a corresponding amount of caloric to the heater, and thereby raise the "working gas," flowing through that receptacle, to the same temperature. This receptacle, with the connecting pipes and the brass cylinder, may be considered as forming one apparatus, under the general designation of the "heater;" *m*, *m* is a pasteboard covering for the heater, and all the parts connected therewith, except the solid brass cylinder, pasteboard being selected as the slowest conducting substance wherewith to surround them.

The working gas, raised to a high temperature, flows down from the heater through the vertical pipes L L L, and impinges on the top surface of the lowest one of two hollow glass cylinders B B. These cylinders are connected together in the centre of their lengths by the bent hollow glass tube C, the whole being exhausted of air, and partially filled with alcohol, as described in the commencement of this paper. A tin collar, S, is attached to the bent tube C, and to a large tin plate S, passing between the two glass cylinders; this plate unites to brass arms or bars, R R, on each side, and by these the whole glass instrument vibrates on pivots, or points of suspension, at the extremities, D D, of two screws, one passed through the side of the cast iron case of the meter, and the other sustained from the bent iron bracket or arm, T, attached to one of the hoods. E is the weight, or bob, fixed to the upper end of the two brass bars, R, to act as a counterbalance in the vacuum, and for the purposes previously described, the pendulous motion thus obtained acting on the train of wheelwork of the registering dials. The use of the tin plate, S′, passing between the two glass cylinders, is to prevent any of the heated gas flowing down upon the lower cylinder from affecting the upper one, and thereby altering the proportionate difference of tem-

perature between the two cylinders, which forms the basis of the principle on which the vibrations are kept up. The "working gas," after acting on the lower cylinder, as above described, flows down through the main body of the meter, and along the outlet at the bottom, where it re-unites with the "neutral gas" coming through the regulating disc Z, and with it passes to the general exit, K, towards the burners.

This, then, is the description of the whole of the mere meter, and it will be evident that no alteration in the measure can, at any time, take place by the leakage of valves, there not being any in the working parts, neither is there therein any membrane or partition, that when such become acted upon, and rendered porous by the action of the gas, any portion can escape through them unmeasured; the moving power being the small light at the top (the "meter jet"), it will likewise be apparent that there is not the least resistance to the flows of the gas, and that thus the gas passes through every part of the meter in the same uninterrupted way it would flow through a pipe, neither interfering with the perfect steadiness of the lights, nor requiring any pressure.

The remaining mechanical contrivances are for stopping-off the gas from the burners when the meter jet is not lighted, for giving a temporary extra temperature to the heater, for a few minutes only, when the meter jet is first lighted, and for the regulation of that jet (when the meter is originally fixed in its place) according to the pressure; the whole contrivances being chiefly, as before observed, for preventing fraud, and ensuring proper registration.

To prevent the gas flowing to the burners, except when the meter jet is lighted, and the meter registering accordingly; a valve which communicates with the supply of gas is opened or shut by the expansion or contraction of a pyrometer, the heat to which is communicated by the small jet at G.

Fig. 1.—Front elevation, with the case of the meter removed.

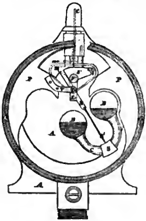


Fig. 2.—Section from the front to the back of the meter.

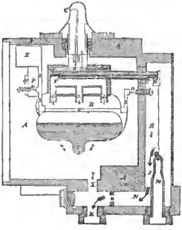


Fig. 3.—Front view of the meter in an ornamental case.

ACTION OF SULPHUROUS ACID ON METALLIC OXIDES.

The following are the results of experiments on the action of sulphurous acid on metallic oxides by M. Vogel.

1st. Red oxide of mercury at first becomes protoxide combined with sulphurous and sulphuric acids, and is afterwards completely reduced to the metallic state by sulphurous acid.

2nd. Pernitrate of mercury is slowly reduced by sulphurous acid, but the reduction becomes perfect with the aid of heat; the protonitrate is reduced in the same manner, but more rapidly.

3rd. Bichloride of mercury is not reduced, under the same circumstances, by sulphurous acid, lower than to protochloride; and when the solution of the bichloride is mixed with a sufficient quantity of sulphurous acid, it is not decomposed by the caustic alkalies added in excess; the mercury remains in solution in the alkaline liquor.

4th. Protochloride of mercury is not reduced to the metallic state by sulphurous acid, but merely to a subchloride of mercury; but subpersulphate of mercury (turbith mineral) is entirely reduced by sulphurous acid.

5th. Neither oxide nor nitrate of silver is completely reduced by sulphurous acid.

6th. The oxides of zinc, antimony and uranium, do not suffer the slightest reduction by sulphurous acid.

7th. The black oxide of copper calcined and left in contact with sulphurous acid, becomes brown protoxide, and the acetate of the oxide becomes acetate of suboxide when heated; the greater part of the copper being deposited in the state of brown suboxide.

8th. Sesquioxide of iron when calcined does not yield any of its

oxygen to sulphurous acid; but the peracetate of iron becomes protoacetate by its action.

9th. Molybdic acid is not reduced by sulphurous acid, but molybdate of potash is reduced to a low state of oxidizement, to the blue compound or molybdous acid.—*Journal de Pharmacie et de Chimie*, Sept. 1843.

EXTRACTION OF PALLADIUM IN BRAZIL.

The extraction of palladium from the auriferous sand of Brazil consists in fusing it with silver, and consequently forming a quaternary alloy of gold, palladium, silver and copper, which is granulated by projecting it into water.

By treating this alloy with nitric acid the gold is separated from the other metals which are soluble in the acid; the silver is precipitated by a solution of common salt in the state of insoluble chloride, which being separated, the copper and palladium are precipitated by plates of zinc. The pulverulent deposit of these metals is redissolved in nitric acid and the solution precipitated by excess of ammonia, which redissolves the oxide of copper and of palladium; when the ammoniacal solution of these metals is saturated with hydrochloric acid, a double chloride of palladium and ammonia is deposited in the state of a crystalline yellow powder, and this when calcined in a crucible is readily decomposed, and leaves spongy palladium — *Journal de Chémie Médical*, October 1843.*

ON THE INFLUENCE OF TEMPERATURE ON THE PRODUCTION OF IODOFORM. BY M. BOUCHARDAT.

Many instances of the influence of temperature in chemical reactions are known, to which M. Bouchardat has added that which it exerts in the production of iodoform.

If to water containing iodide of potassium and a little alcohol, iodine and potassium be alternately added in sufficient quantity to decolorize and colour the liquor, it becomes hot, and there are successively produced acetic æther, and iodoform without any trace of iodate of potash.

But if instead of this the iodine be dissolved in the water and alcohol holding iodide of potassium in solution, and to these there be added an aqueous solution of potash, but not in sufficient quantity to decolorize the liquors, and then fresh iodine be added and successively potash in sufficient quantity to decolorize, then no trace of iodoform is produced. The oxygen which the iodine displaces from the potash acts upon the alcohol to convert it into acetic æther, which may be easily separated; but the action goes no further; as soon as the alcohol is converted into acetic æther, the oxygen displaced acts upon the iodine to produce iodate of potash which is deposited.

Thus with excess of iodine at common temperatures, there is no

* See p. 16 of the present volume.

production of iodoform by the mutual reaction of iodine upon alcohol, under the influence of potash; acetic æther only is formed.

If, on the other hand, to an aqueous solution of carbonate of potash there be added alcohol, iodide of potassium, and iodine in excess (the alcohol not being sufficient to separate the saline solution), and the mixture be exposed to a temperature of 140° Fahr., there is an abundant production of iodoform in a few hours, iodine in excess always remaining.

M. Bouchardat observes, that it was long before he could account for the difference of action under circumstances which appeared so similar, for while with caustic potash acetic æther only was obtained, whereas with the carbonate the product was iodoform; it afterwards appeared that as it was requisite to employ a temperature of 140° with the carbonate of potash, whilst with the caustic potash it was only about 60° to 68° Fahr., this difference might account for the different effects produced. To determine this a solution of iodine and iodide of potassium in water and alcohol was heated to 140° Fahr., and a solution of caustic potash was then added, in which the formation of iodoform immediately took place; it is therefore evident that the different reactions were occasioned by variations of temperature. —*Journal de Pharmacie et de Chimie*, Juillet, 1843.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1843.

Chiswick.—Sept. 1. Foggy: sultry. 2, 3. Slight haze: sultry. 4. Clear and fine. 5. Heavy dew: clear. 6. Cloudless. 7. Slight haze: cloudless and hot. 8, 9. Very fine. 10. Foggy: heavy thunder-showers. 11. Very fine. 12. Overcast. 13. Clear and fine. 14. Overcast. 15—20. Exceedingly fine. 21. Foggy: very fine. 22, 23. Clear and fine. 24, 25. Overcast. 26. Fine: clear and cool. 27. Cloudy and cool: clear, with slight frost at night. 28. Very clear: overcast. 29. Cold and dry: overcast. 30. Rain.—Mean temperature of the month 3°·81 above the average.

Boston.—Sept. 1—6. Fine. 7. Fine: quarter-past 2 P.M. heat 77°. 8. Foggy. 9. Cloudy. 10. Fine: rain P.M. 11, 12. Cloudy. 13—15. Fine. 16. Cloudy. 17—19. Fine. 20. Cloudy: rain early A.M. 21, 22. Fine. 23, 24. Foggy. 25. Cloudy: rain A.M. 26. Windy. 27. Cloudy. 28. Windy. 29. Cloudy. 30. Cloudy: rain early A.M.

Sandwick Manse, Orkney.—Sept. 1. Clear. 2. Cloudy: showers. 3. Showers. 4. Showers: cloudy. 5. Damp: drizzle. 6. Damp: fine. 7—9. Clear: hot: fine. 10. Damp. 11. Haze: fog. 12. Fine. 13. Haze: clear. 14. Clear. 15. Clear: cloudy. 16. Clear. 17. Cloudy: fine: damp. 18. Showers. 19. Clear: aurora. 20. Rain. 21. Showers: cloudy. 22. Cloudy. 23. Damp: drizzle. 24. Drizzle. 25. Showers: drizzle. 26. Bright: cloudy: aurora. 27. Showers. 28. Showers: cloudy. 29. Rain. 30. Cloudy: rain.

Applegarth Manse, Dumfries-shire.—Sept. 1. Fair and fine: one slight shower. 2. Fair and fine. 3. Showery. 4, 5. Fine harvest-day. 6. Fine harvest-day: one slight shower. 7. Fine harvest-day: fair. 8, 9. Fine harvest-days. 10. Fine harvest-day, but cloudy. 11. Fine: shower early A.M. 12, 13. Fine harvest-days. 14. Fine harvest-day: thunder. 15. Fine harvest-day. 16. Fine harvest-day: sheet lightning. 17. Showery. 18. Fair and fine: thunder. 19. Fair and fine. 20—24. Fine harvest-day. 25. Fine harvest-days: hoar-frost. 26. Fine harvest-day: no frost. 27. Fine harvest-day: hoar-frost. 28. Dull: wet evening. 29. Cloudy: rain. 30. Cloudy.

Sun shone out 28 days. Rain fell 7 days. Thunder 2 days. Hoar-frost 2 days.

Calm 14 days. Moderate 9 days. Brisk 4 days. Strong breeze 3 days.

Mean temperature of the month .... 56°·3

Meteorological Observations made at the Apartments of the Royal Society, LONDON, *by the Assistant Secretary,* Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London*; *by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse,* DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month. 1843. Sept.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8¾ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 9 p.m.	Barometer. Orkney, Sandwick. 9¼ a.m.	Barometer. Orkney, Sandwick. 8¾ p.m.	Thermometer. London: R.S. Fahr. 9 a.m.	Thermometer. London: R.S. Self-reg. Mx.	Thermometer. London: R.S. Self-reg. Min.
☽ 1.	30·378	30·284	29·943	29·70	30·19	30·25	30·15	30·22	67·8	73·6	61·3
2.	30·428	30·333	30·117	29·75	30·24	30·24	30·15	30·08	69·7	78·3	64·0
3.	30·374	30·281	30·200	29·60	30·09	30·11	29·75	29·81	67·5	77·0	62·5
4.	30·356	30·330	30·256	29·64	30·21	30·30	30·05	30·22	62·5	78·7	60·3
5.	30·472	30·388	30·312	29·84	30·33	30·25	30·20	30·16	58·7	68·4	55·2
6.	30·368	30·292	30·251	29·73	30·22	30·20	30·11	30·20	61·3	68·0	54·3
7.	30·354	30·270	30·246	29·69	30·21	30·20	30·20	30·23	62·7	73·8	60·3
○ 8.	30·354	30·267	30·193	29·70	30·19	30·15	30·25	30·25	66·3	75·6	60·0
9.	30·244	30·183	30·086	29·65	30·13	30·07	30·22	30·04	65·3	73·3	63·6
10.	30·092	30·022	29·956	29·46	29·99	29·80	30·00	29·93	63·0	74·0	60·2
11.	30·026	30·075	29·942	29·38	30·04	30·10	30·00	30·10	63·5	73·0	61·0
12.	30·272	30·200	30·183	29·66	30·25	30·24	30·21	30·21	61·5	69·0	59·5
13.	30·204	30·133	29·931	29·62	30·20	30·04	30·16	30·05	65·3	73·5	55·4
14.	29·912	29·847	29·826	29·40	29·90	29·88	30·00	30·00	61·5	69·7	54·7
15.	29·888	29·956	29·796	29·33	29·89	29·87	29·98	29·92	68·7	71·3	61·7
☾ 16.	30·076	30·053	29·994	29·43	29·90	29·98	29·88	29·86	67·7	73·0	61·0
17.	30·216	30·128	30·082	29·54	30·05	30·03	29·96	29·93	68·3	76·7	59·7
18.	30·192	30·148	30·090	29·44	29·93	30·10	29·70	29·94	66·7	78·0	63·0
19.	30·256	30·163	30·148	29·60	30·19	30·12	30·20	30·12	67·2	74·4	62·7
20.	30·190	30·167	30·034	29·54	30·00	29·96	29·87	29·79	64·7	74·8	58·3
21.	30·226	30·216	30·131	29·54	30·04	30·20	30·01	30·19	60·8	73·4	57·4
22.	30·468	30·446	30·303	29·83	30·37	30·43	30·27	30·40	60·2	71·6	57·0
● 23.	30·590	30·509	30·460	30·00	30·47	30·46	30·44	30·44	58·8	68·7	54·0
24.	30·542	30·467	30·347	29·92	30·42	30·36	30·31	30·31	59·0	66·4	56·2
25.	30·316	30·259	30·161	29·76	30·23	30·22	30·27	30·23	55·3	64·2	52·3
26.	30·186	30·064	29·947	29·63	30·13	29·90	30·11	29·92	52·3	65·2	46·6
27.	29·806	29·781	29·681	29·27	29·74	29·73	29·81	29·82	49·2	57·0	44·6
28.	29·770	29·834	29·735	29·32	29·78	29·83	29·82	29·88	47·5	54·0	41·8
29.	30·004	29·968	29·937	29·50	29·85	29·75	29·80	29·50	48·3	56·2	41·4
☽ 30.	29·814	29·893	29·788	29·16	29·79	29·75	29·61	29·51	55·6	57·0	49·3
Mean.	30·212	30·162	30·071	29·58	30·098	30·094	30·049	30·042	61·6	70·3	56·6

Days of Month. 1843. Sept.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8¾ a.m.	Thermometer. Dumf.-shire. Max.	Thermometer. Dumf.-shire. Min.	Thermometer. Orkney, Sandwick. 9¼ a.m.	Thermometer. Orkney, Sandwick. 8¾ p.m.	Wind. London: R.S. 9 a.m.	Wind. Chiswick. 1 p.m.	Wind. Boston.	Wind. Dumfries-shire.	Wind. Orkney, Sandwick.	Rain. London: R.S. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Rain. Orkney, Sandwick.	Dew-point. Lond. R.S. 9 a.m.
☽ 1.	85	57	66	67½	54	56	52	e.	s.	calm	s.	w.	......	......	......	......	......	63
2.	82	54	65	68	58	58½	60	nnw.	n.	w.	ssw.	s.	......	......	......	......	......	65
3.	83	57	70	67	59	63	54	nw.	sw.	calm	w.	sw.	......	......	......	......	·29	65
4.	75	42	60	60	50½	55	53	w.	n.	nw.	n & w.	sw.	......	......	......	......	·12	60
5.	74	45	58·5	67	41½	54	55	n.	n.	calm	s & w.	w.	......	......	......	......	......	52
6.	81	50	62	70½	48½	56½	57	n.	nw.	calm	sw.	w.	......	......	......	......	·09	57
7.	83	53	66·5	69½	45	63	58	nnw.	s.	calm	sw.	sw.	......	......	......	0·10	......	60
○ 8.	78	62	62	75	43½	62	59	e.	e.	calm	sw.	sw.	......	......	......	......	......	63
9.	79	55	61	77	48½	69	57	ene.	e.	se.	sw.	s.	......	......	......	......	......	64
10.	74	56	66·5	72½	50	57	55	ene.	s.	calm	e.	se.	......	·52	......	......	......	62
11.	74	58	61	73½	51	58	57	nw.	w.	calm	e.	se.	·250	......	·18	......	......	61
12.	75	51	62	74	54	60½	57½	nne.	ne.	calm	se.	...	......	......	......	......	......	61
13.	72	47	63	70½	51½	58½	55	ne.	e.	se.	e.	s.	......	......	......	......	......	61
14.	69	62	62	70½	47	56	54	e.	e.	calm	e.	se.	......	·01	......	0·02	......	59
15.	76	54	65	74½	55	58	54½	e.	s.	se.	ne.	se.	......	......	......	......	......	63
☾ 16.	83	53	67	71½	54	60½	57	e.	s.	calm	ne.	se.	......	......	......	......	......	61
17.	84	52	66	69	57	59½	59	ssw.	sw.	calm	s.	s.	......	......	......	......	......	64
18.	80	58	65·5	65	52	54½	61	sse.	nw.	calm	s.	sw.	......	·20	......	......	·36	64
19.	80	49	65	64½	41	54½	53	n.	ne.	calm	w.	s.	·197	......	......	......	·08	65
20.	81	47	66	67	55	56	58	ne.	se.	calm	sw.	s.	......	......	·17	......	·11	62
21.	75	50	63	65	57	53	54½	e.	sw.	calm	sw.	nw.	......	......	......	......	·16	61
22.	71	48	61	68½	40½	57	55	n.	ne.	calm	sw.	w.	......	......	......	0·03	......	60
● 23.	69	45	57	69½	42	57	55	n.	ne.	calm	e.	w.	......	......	......	......	......	59
24.	66	45	53	64	50	56	49½	n.	nw.	wnw.	nw.	w.	......	......	......	......	·03	59
25.	65	41	57	59	48	44½	44	s.	nw.	nw.	nw.	nw.	......	......	......	......	·01	57
26.	60	38	50	56½	32	47	44½	n.	nw.	nw.	n.	nw.	......	......	·05	......	·06	52
27.	58	32	47	54	41½	44½	43	w.	nw.	nw.	n.	n.	......	......	......	......	·13	49
28.	59	34	45	56½	35½	45	46	nw.	nw.	nw.	n.	nw.	......	......	......	......	·07	42
29.	62	47	46	53	30½	50½	51	w.	nw.	nw.	n.	nw.	......	·24	......	0·04	·24	43
☽ 30.	68	55	56	61	48	51½	51	wsw.	w.	w.	sw.	n.	·218	·01	·07	......	·21	48
Mean.	73·90	49·90	60·5	66·3	46·7	55·86	53·63						Sum. ·665	0·98	0·47	0·19	2·00	Mean. 59

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XLIX. *Description of the Tithonometer, an instrument for measuring the Chemical Force of the Indigo-tithonic Rays. By* John W. Draper, *M.D., Professor of Chemistry in the University of New York**.

I HAVE invented an instrument for measuring the chemical force of the tithonic rays which are found at a maximum in the indigo space, and which from that point gradually fade away to each end of the spectrum. The sensitiveness, speed of action and exactitude of this instrument, will bring it to rank as a means of physical research with the thermo-multiplier of M. Melloni.

The means which have hitherto been found available in optics for measuring intensities of light, by a relative illumination of spaces or contrast of shadows, are admitted to be inexact. The great desideratum in that science is a photometer which can mark down effects by movements over a graduated scale. With those optical contrivances may be classed the methods hitherto adopted for determining the force of the tithonic rays by stains on Daguerrotype plates or the darkening of sensitive papers. As deductions, drawn in this way, depend on the *opinion* of the observer, they can never be perfectly satisfactory, nor bear any comparison with thermometric results.

Impressed with the importance of possessing for the study of the properties of the tithonic rays some means of accurate measurement, I have resorted in vain to many contrivances; and, after much labour, have obtained at last the instrument which it is the object of this paper to describe.

The tithonometer consists essentially of a mixture of equal measures of chlorine and hydrogen gases, evolved from and confined by a fluid which absorbs neither. This mixture is kept in a graduated tube, so arranged that the gaseous surface

* Communicated by the Author.

exposed to the rays never varies in extent, notwithstanding the contraction which may be going on in its volume, and the muriatic acid resulting from its union is removed by rapid absorption.

The theoretical conditions of the instrument are therefore sufficiently simple; but, when we come to put them into practice, obstacles which appear at first sight insurmountable are met with. The means of obtaining chlorine are all troublesome; no liquid is known which will perfectly confine it; it is a matter of great difficulty to mix it in the true proportion with hydrogen, and have no excess of either. Nor is it at all an easy affair to obtain pure hydrogen speedily, and both these gases diffuse with rapidity through water into air.

Without dwelling further on the long catalogue of difficulties which is thus to be encountered, I shall first give an account of the capabilities of the instrument in the form now described, which will show to what an extent all those difficulties are already overcome. In a course of experiments on the union of chlorine and hydrogen, some of which were read at the last meeting of the British Association, I found that the sensitiveness of that mixture had been greatly underrated. The statement made in the books of chemistry, that artificial light will not affect it, is wholly erroneous. The feeblest gleams of a taper produce a change. No further proof of this is required than the tables given in this communication, in which the radiant source was an oil-lamp. For speed of action no tithonographic compound can approach it; a light, which perhaps does not endure the millionth part of a second, affects it energetically, as will be hereafter shown.

Proofs of the sensitiveness of the Tithonometer.—The following illustrations will show that the tithonometer is promptly affected by rays of the feeblest intensity, and of the briefest duration.

When, on the sentient tube of the tithonometer, the image of a lamp formed by a convex lens is caused to fall, the liquid instantly begins to move over the scale, and continues its motion as long as the exposure is continued. It does not answer to expose the tube to the direct emanations of the lamp without first absorbing the radiant heat, or the calorific effect will mask the true result. By the interposition of a lens this heat is absorbed, and the tithonic rays alone act.

If a tithonometer is exposed to daylight coming through a window, and the hand or a shade of any kind is passed in front of it, its movement is *in an instant* arrested; nor can the shade be passed so rapidly that the instrument will fail to give the proper indication.

The experimenter may further assure himself of the extreme sensitiveness of this mixture by placing the instrument before a window, and endeavouring to remove and replace its screen so quickly that it shall fail to give any indication; he will find that it cannot be done.

Charge a Leyden phial, and place the tithonometer at a little distance from it, keeping the eye steadily fixed on the scale; discharge the jar, and the rays from the spark will be seen to exert a very powerful effect, the movement taking place and ceasing in an instant.

This remarkable experiment not only serves to prove the sensitiveness of the tithonometer, but also brings before us new views of the powers of that extraordinary agent electricity. That energetic chemical effects can thus be produced at a distance by an electric spark in its momentary passage, effects which are of a totally different kind from the common manifestations of electricity, is thus proved; these phænomena being distinct from those of induction or molecular movements taking place in the line of discharge, they are of a radiant character, and due to the emission of tithonicity; and we are led at once to infer that the well-known changes brought about by passing an electric spark through gaseous mixtures, as when oxygen and hydrogen are combined into water, or chlorine and hydrogen into muriatic acid, arise from a very different cause than those condensations and percussions by which they are often explained, a cause far more purely chemical in its kind. If chlorine and hydrogen can be made to unite silently by an electric spark passing outside the vessel which contains them, at a distance of several inches, there is no difficulty in understanding why a similar effect should take place with a violent explosion when the discharge is made through their midst; nor how a great many mixtures may be made to unite under the same treatment. A flash of lightning cannot take place, nor an electric spark be discharged, without chemical charges being brought about by the radiant matter emitted.

Proofs of the exactness of the indications of the Tithonometer.—The foregoing examples may serve to illustrate the extreme sensitiveness of the tithonometer; I shall next furnish proofs that its indications are exactly proportional to the quantities of light incident on it.

As it is necessary, owing to the variable force of daylight, to resort to artificial means of illumination, it will be found advantageous to employ the following method of obtaining a flame of suitable intensity.

Let A B, fig. 4, be an Argand oil lamp of which the wick is C. Over the wick, at a distance of half an inch or thereabouts, place a plate of thin sheet copper, three inches in diameter, perforated in its centre with a circular hole of the same diameter as the wick, and concentric therewith. This piece of copper is represented at *d d*; it should have some contrivance for raising or depressing it through a small space, the proper height being determined by trial. On this plate, the glass cylinder *e*, an inch and three-quarters in diameter and eight or ten inches long, rests.

When the lamp is lighted, provided the distance between the plate *d d* and the top of the wick is properly adjusted, on putting on the glass cylinder the flame instantly assumes an intense whiteness; by raising the wick it may be elongated to six inches or more, and becomes exceedingly brilliant. Lamps constructed on these principles may be purchased in the shops. I have, however, contented myself with using a common Argand study-lamp, supporting the perforated plate *d d* at a proper altitude by a retort stand. It will be easily understood that the great increase of light arises from the circumstance that the flame is drawn violently through the aperture in the plate by the current established in the cylinder.

As much radiant heat is emitted by this flame, in order to diminish its action, and also to increase the tithonic effect, I adopt the following arrangement. Let A B, fig. 4, be the lamp; the rays emitted by it are received on a convex lens D, four inches and three-quarters in diameter, that which I use being the large lens of a lucernal microscope. This, placed at a distance of twenty-one inches from the lamp, gives an image of the flame at a distance of thirteen inches, which is received on the sentient tube of the tithonometer F; between the tithonometer and the lens there is a screen E.

Things being thus arranged, and the lamp lighted so as to give a flame about three inches and a half long, the experiments may be proceeded with. It is convenient always to work with the flame at a constant height, which may be determined by a mark on the glass cylinder. At a given instant, by a seconds watch, the screen E is removed, and immediately the tithonometer begins to descend. When the first minute is elapsed the position on the scale is read off and registered; at the close of the second minute the same is done, and so on with the third, &c. And now, if those numbers be compared, casting aside the first, they will be found equal to one another, as the following table of experiments, made at different times and with different instruments, shows:—

TABLE I.

Showing that when the radiant source is constant, the amount of movement in the tithonometer is directly proportional to the times of exposure.

Time.	Experiments.				
	1.	2.	3.	4.	5.
30″	7·00	7·00	10·25	...	5·25
60	8·00	7·75	11·50	11·75	6·50
90	7·50	8·00	11·50	...	6·25
120	7·75	7·75	11·50	13·00	6·00
150	7 75	7·25	...	...	6·00
180	...	...	...	12·00	6·00
210	...	...	...	...	6·00
Mean	7·60	7·55	11·19	12·25	6·00

From this it will be perceived that, taking the first experiment as an example, if at the end of 30 seconds the tithonometer has moved 7·00, at the end of 60″ it has moved 8·00 more, at the end of 90″, 7·50 more, at the end of 120″, 7·75 more; the numbers set down in the vertical column representing the amount of motion for each thirty seconds. And, when it is recollected that the readings are all made with the instrument in motion, the differences between the numbers do not greatly exceed the possible errors of observation. It may be remarked that the third and fourth experiments were made with a different lamp.

Though a certain amount of radiant heat from a source so highly incandescent as that here used will pass the lens, its effects can never be mistaken for those of the tithonic rays. This is easily understood when we remember that the effect of such transmitted heat would be to expand the gaseous mixture, but the tithonic effect is to contract it.

Next, I shall proceed to show that the indications of the tithonometer are strictly proportional to the quantity of rays that have impinged upon it; a double quantity producing a double effect, a triple quantity a threefold effect, &c.

A slight modification in the arrangement (fig. 4) enables us to prove this in a satisfactory way. The lens D, being mounted in a square wooden frame, can easily be converted into an instrument for delivering at its focal point, where the sentient tube is placed, measured quantities of the tithonic rays, and thus becomes an invaluable auxiliary in those researches which require known and predetermined quantities of tithonicity to

be measured out. The principle of the modification is easily apprehended. If half the surface of the lens be screened by an opake body, as a piece of blackened cardboard, of course only half the quantity of rays will pass which would have passed had the screen not been interposed. If one-fourth of the lens be left uncovered, only one-fourth of the quantity will pass; but in all these instances the focal image remains the same as before. By adjusting, therefore, upon the wooden frame of the lens, two screens the edges of which pass through its centre, and are capable of rotation upon that centre, we shall cut off all light when the screens are applied edge to edge, we shall have 90° when they are rotated so as to be at right angles, and 180° when they are superposed with their edges parallel. Thus by setting them in different angular positions, we can gain all quantities from 0° up to 180°, and by removing them entirely away reach 360°.

It will be understood that the effect of the instrument is to give an image of a visible object of which the intensity can be made to vary at pleasure in a known proportion.

In order therefore to prove that the indications of the tithonometer are proportional to the quantity of impinging rays, place this *measuring lens* in the position D, setting its screens at an angle of 90°. Remove the screen E, and determine the effect on the tithonometer for one minute. At the close of the minute, and without loss of time, turn one of the screens so as to give an angle of 180°, and now the effect will be found double what it was before, as in the following table:—

Table II.

Showing that the indications of the tithonometer are proportional to the quantity of incident rays.

Quantities.	Experiment 1. Observed.	Experiment 1. Calculated.	Experiment 2. Observed.	Experiment 2. Calculated.
90°	2·18	2·22	2·69	2·75
180	4·27	4·45	5·75	5·50
270	6·70	6·67	8·25	8·25
360	8·90	8·90	11·00	11·00

I have stated in the commencement of this paper, that the action upon the tithonometer is limited to a ray which corresponds in refrangibility to the indigo, or rather, that in the indigo space its maximum action is found. The following table serves at once to prove this fact, and also to illustrate the chemical force of the different regions of the spectrum:—

TABLE III.

Showing that the maximum for the tithonometer is in the indigo space of the spectrum.

Space.	Ray.	Force.	Space.	Ray.	Force.
0	Extreme red	·33	8	Blue-indigo...	204·00
1	Red	·50	9	Indigo	240·00
2	Orange......	·75	10	Violet	121·00
3	Yellow......	2·75	11	Violet	72·00
4	Green	10·00	12	Violet	48·00
5	Green-blue	54·00	13	Violet	24·00
6	Blue.........	108·00	14	Extra-spectral	12·00
7	Blue.........	144·00			

In this table the spaces are equal; the centre of the red, as insulated by cobalt blue glass, is marked as unity; the centre of the yellow, insulated by the same, being marked 3; the intervening region being divided into two equal spaces, and divisions of the same value carried on to each end of the spectrum.

As instruments will no doubt be hereafter invented for measuring the phænomena of different classes of rays, it may prove convenient to designate the precise ray to which they apply. Perhaps the most simple mode is to affix the name of the ray itself. Under that nomenclature the instrument described in this paper would take the name of Indigo-tithonometer.

There is no difficulty in adapting this instrument to the determination of questions relating to absorption, reflexion and transmission. Thus I found that a piece of colourless French plate-glass transmitted 866 rays out of 1000.

Description of the Instrument. First, of the glass part.— The tithonometer consists of a glass tube bent into the form of a siphon, in which chlorine and hydrogen can be evolved from muriatic acid, containing chlorine in solution, by the agency of a voltaic current. It is represented by fig. 1, where *a b c* is a clear and thin tube four-tenths of an inch external diameter, closed at the end *a*. At *d* a circular piece of metal, an inch in diameter, which may be called the stage, is fastened on the tube, the distance from *d* to *a* being 2·9 inches. At the point *x*, which is two inches and a quarter from *d*, two platina wires, *x* and *y*, are fused into the glass, and entering into the interior of the tube, are destined to furnish the supply of chlorine and hydrogen; from the stage *d* to the point *b*, the inner bend of the tube, is 2·6 inches, and from that

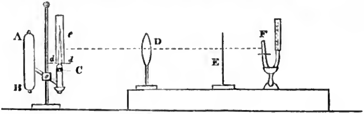


Fig. 4.

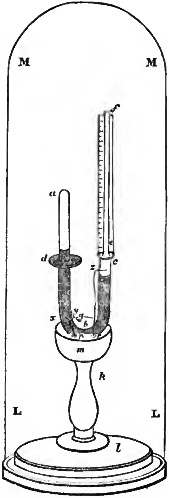


Fig. 1.

The Tithonometer.

Fig. 3.

Fig. 2.

point to the top of the siphon *c*, the distance is three inches and a half. Through the glass at *z*, three quarters of an inch from *c*, a third platina wire is passed; this wire terminates in the little mercury cup *r*, and *x* and *y* in the cups *p* and *q* respectively.

Things being thus arranged, the instrument is filled with its fluid prepared, as will presently be described; and as the legs *a b*, *b c* are not parallel to each other, but include an angle of a few degrees, in the same way that Ure's eudiometer is arranged, there is no difficulty in transferring the liquid to the sealed leg. Enough is admitted to fill the sealed leg and the open one partially, leaving an empty space to the top of the tube at *c* of two and three quarter inches.

A stout tube, six inches long and one-tenth of an inch interior diameter, *e f*, is now fused on at *c*. Its lower end opens into the main siphon tube; its upper end is turned over at *f*, and is narrowed to a fine termination, so as barely to admit a pin, but is not closed. This serves to keep out dust, and in case of a little acid passing out, it does not flow over the scale and deface the divisions. At the back of this tube a scale is placed, divided into tenths of an inch, being numbered from above downwards. Fifty of these divisions are as many as will be required. Fig. 2 shows the termination of the narrow tube bent over the scale.

From a point one-fourth of an inch above the stage *d*, downwards beyond the bend, and to within half an inch of the wire *z*, the whole tube is carefully painted with India ink so as to allow no light to pass; but all the space from a fourth of an inch above the stage *d* to the top of the tube *a*, is kept as clear and transparent as possible. This portion constitutes the sentient part of the instrument. A light metallic or pasteboard cap, A D, fig. 3, closed at the top and open at the bottom, three inches long and six-tenths of an inch in diameter, blackened on its interior, may be dropped over this sentient tube; it being the office of the stage *d* to receive the lower end of the cap when it is dropped on the tube so as to shut out the light.

The foot of the instrument *k l* is of brass, it screws into the hemispherical block *m*, which may be made of hard wood or ivory; in this three holes, *p q r*, are made to serve as mercury cups; they should be deep and of small diameter, that the metal may not flow out when it inclines for the purpose of transferring. A brass cylindrical cover L M, L M may be put over the whole; when it is desirable to preserve it in total darkness, it should be blackened without.

Secondly, of the Fluid Part.—The fluid from which the mixture of chlorine and hydrogen is evolved, and by which it is

confined, is yellow commercial muriatic acid, holding such a quantity of chlorine in solution that it exerts no action on the mixed gases as they are produced. From the mode of its preparation it always contains a certain quantity of chloride of platina, which gives it a deep golden colour, a condition of considerable incidental importance.

When muriatic acid is decomposed by voltaic electricity its chlorine is not evolved, but is taken up in very large quantity and held in solution; perhaps a bichloride of hydrogen results. If through such a solution hydrogen gas is passed in minute bubbles, it removes with it a certain proportion of the chlorine. From this therefore it is plain, that muriatic acid thus decomposed will not yield equal measures of chlorine and hydrogen unless it has been previously impregnated with a certain volume of the former gas. Nor is it possible to obtain that degree of saturation by voltaic action, no matter how long the electrolysis is continued, if the hydrogen is allowed to pass through the liquid.

Practically, therefore, to obtain the tithonometric liquid, we are obliged to decompose commercial muriatic acid in a glass vessel, the positive electrodes being at the bottom of the vessel and the negative at the surface of the liquid. Under these circumstances, the chlorine as it is disengaged is rapidly taken up, and the hydrogen being set free without its bubbles passing through the mass, the impregnation is carried to the point required.

Although this chlorinated muriatic acid cannot of course be kept in contact with the platina wires without acting on them, the action is much slower than might have been anticipated. I have examined the wires of tithonometers that had been in active use for four months, and could not perceive the platina sensibly destroyed. It is well however to put a piece of platina foil in the bottle in which the supply of chlorinated muriatic acid is kept; it communicates to it slowly the proper golden tint.

The liquid, being impregnated with chlorine in this manner until it exhales the odour of that gas, is to be transferred to the siphon *a b c* of the tithonometer, and its constitution finally adjusted as hereafter shown.

Thirdly, of the Voltaic Battery.—The battery, which will be found most applicable for these purposes, consists of two Grove's cells, the zinc surrounding the platina.

The following are the dimensions of the pairs which I use. The platina plate is half an inch wide and two inches long; it dips into a cylinder of porous biscuit-ware of the same dimensions, which contains nitric acid. Outside this porous

vessel is the zinc, which is a cylinder one inch diameter, two inches long and two-tenths thick; it is amalgamated. The whole is contained in a cup, two inches in diameter, and two deep, which also receives the dilute sulphuric acid.

The force of this battery is abundantly sufficient both for preparing the fluid originally and for carrying on the tithonometric operations; it can decompose muriatic acid with rapidity, and will last with ordinary care for a long time.

Before passing to the mode of using the tithonometer, it is absolutely necessary to understand certain theoretical conditions of its equilibrium; to these in the next place I shall revert.

Theoretical Conditions of Equilibrium.—The tithonometer depends for its sensitiveness on the exact proportion of the mixed gases. If either one or the other is in excess a great diminution of delicacy is the result. The comparison of its indications at different times depends on the certainty of evolving the gases in exact, or at all events, known proportions.

Whatever, therefore, affects the constitution of the sentient gases alters at the same time their indications. Between those gases and the fluid which confines them certain relations subsist, the nature of which can be easily traced. Thus, if we had equal measures of chlorine and hydrogen, and the liquid not saturated with the former, it would be impossible to keep them without change, for by degrees a portion of chlorine would be dissolved, and an excess of hydrogen remain; or, if the liquid was overcharged with chlorine, an excess of that gas would accumulate in the sentient tube.

It is absolutely necessary, therefore, that there should be an equilibrium between the gaseous mixture and the confining fluid.

As has been said, when muriatic acid is decomposed by a voltaic current, all the chlorine is absorbed by the liquid and accumulates therein, the hydrogen bubbles however as they rise withdraw a certain proportion, and hence pure hydrogen passed up through the tithonometric fluid becomes exceedingly sensitive to the light.

There are certain circumstances connected with the constitution and use of the tithonometer which continually tend to change the nature of its liquid. The platina wires immersed in it by slow degrees give rise to a chloride of platina. It is true that this takes place very gradually, and by far the most formidable difficulty arises from a direct exhalation of chlorine from the narrow tube *e f*, for each time that the liquid descends, a volume of air is introduced, which receives a cer-

tain amount of chlorine which with it is expelled the next time the battery raises the column to zero; and this, going on time after time, finally impresses a marked change on the liquid. I have tried to correct this in various ways, as by terminating the end *f* with a bulb; but this entails great inconvenience, as may be discovered by any one who will reflect on its operation.

When by the battery we have raised the index to its zero point, if the gas and liquid are not in equilibrio, that zero is liable to a slight change. If there be hydrogen in excess the zero will rise,—if chlorine, the zero will fall.

In making what will be termed "interrupted experiments," we must not too hastily determine the position of the index on the scale at the end of a trial. It is to be remembered that the cause of movement over the scale arises from a condensation of muriatic acid, but that condensation, though very rapid, is not instantaneous. Where time is valuable, and the instrument in perfect equilibrium, this condensation may be instantaneously effected, by simply inclining the instrument so that its liquid may pass down to the closed end *a*, but not so much as to allow gas to escape into the other leg; the inclination of the two legs to each other makes this a very easy manipulation, and the gas thus brought into contact with an extensive liquid surface yields up its muriatic acid in a moment.

Directions for using the Tithonometer. Preliminary adjustment.—Having transferred the liquid to the sealed end of the siphon, and placed the cap on the sentient extremity, the voltaic battery being prepared, the operator dips its polar wires into the cups *p q*, which are in connexion with the wires *x y*. Decomposition immediately takes place, chlorine and hydrogen rising through the liquid, and gradually depressing it, whilst of course a corresponding elevation takes place in the other limb; this operation is continued until the liquid has risen to the zero. It takes but a few seconds for this to be accomplished.

The polar wires having been disengaged, the tithonometer is removed opposite a window, care being taken that the light is not too strong. The cap is now lifted off the sentient extremity *a d*, and immediately the liquid descends. This exposure is allowed to continue, and the liquid suffered to rise as much as it will to the end *a*. And now, if the gases have been properly adjusted, an entire condensation will take place, the sentient tube *a d* filling completely. In practice this precision is not however obtained, and if a bubble as large as a

peppercorn be left, the operator will be abundantly satisfied with the sensitiveness of his instrument. Commonly, at first, a large residue of hydrogen gas, occupying perhaps an inch or more, will be left. It is to be understood that even this large surplus will disappear in a few hours by absorbing chlorine. But this is not to be waited for; as soon as no further rise takes place in a minute or two, the siphon is to be inclined on one side, and the residue turned out into the open leg.

Now, recurring to what has been said on the equilibrium, it is plain that this excess of hydrogen arises from a want of chlorine in the tithonometric liquid. A proper quantity must therefore be furnished by proceeding as follows.

The sentient tube being filled with the liquid by inclination, connect the polar wires with $p\,q$, as before. These may be called *generating wires.* Allow the liquid to rise in $b\,c$, until the third platina wire z, which may be called the *adjusting wire*, is covered an eighth of an inch deep. Then remove the negative wire from the cup p into the cup r, and now the conditions for saturating the liquid are complete; hydrogen escaping away from the surface of the liquid at z, and chlorine continually accumulating and dissolving between x and d. This having been carried on for a short time, the gas in $a\,d$ is to be turned out by inclination and the instrument recharged. That a proper quantity is evolved is easily ascertained by allowing total condensation to take place, and observing that only a small bubble is left at a.

It will occasionally happen in this preliminary adjustment, that an excess of chlorine may arise from continuing the process too long. This is easily discovered by its greenish-yellow tint, and is to be removed by inclining the instrument and turning it out.

Thus adjusted, everything is ready to obtain measures of any effect, there being two different methods by which this can be done,—1st, by continuous observation; 2nd, by interrupted observation.

Of the Method of continuous observation.—This is best described by resorting to an example. Suppose, therefore, it is required to verify Table I., or, in other words, to prove that the effect on the tithonometer is proportional to its time of exposure.

Put on the cap of the sentient tube $a\,d$, connect the polar wires with $p\,q$, and raise the liquid to zero.

Place the tithonometer so that its sentient tube will receive the rays properly.

At a given instant, marked by a seconds watch, remove the cap A D, and the liquid at once begins to descend. At the end of the first minute read off the division over which it is

passing. Suppose it is 7. At the end of the second do the same, it should be 14; at the end of the third 21, &c. &c. This may be done until the fiftieth division is reached, which is the terminus of the scale.

Recharge the tube by a momentary application of the polar wires: but it is convenient first to remove any excess of muriatic acid gas in the sentient tube by allowing it time for condensation; or, if that be inadmissible, by inclining a little on one side, so as to give an extensive liquid contact.

Of the Method of interrupted observation.—It frequently happens that observations cannot be had during a continuous descent, as when changes have to be made in parts of apparatus or arrangements. We have then to resort to interrupted observations.

This method requires that the gas and liquid should be well adjusted, so that no change can arise in volume when extensive contact is made by inclination.

The tithonometer being charged, place it in a proper position. At a given instant remove its cap, and the liquid descends. When the time marked by a seconds watch has elapsed, drop the cap on the sentient tube. The liquid simultaneously pauses in its descent, but does not entirely stop, for a little uncondensed muriatic acid still exists, which is slowly disappearing in the sentient tube. Now, incline the instrument for a moment on one side, so that the liquid may run up to the cord *a*, but not so much as to let any gas escape. Restore it to its position and read off on the scale. It is then ready for a second trial.

The difference between continuous and interrupted observation is this, that in the latter we pause to wash out the muriatic acid, and though this is effected by the simplest of all possible methods, continuous observations are always to be preferred when they can be obtained.

I have extended this paper to so great a length that many points on which remarks might have been made must be passed over. It is scarcely necessary to say that the sentient tube must be *uniformly* and perfectly clean. As a general rule also, the first observation may be cast aside, for reasons which I will give hereafter. Further, it is to be remarked, as it is an essential principle that during different changes of volume of the gas its exposed surface must never vary in extent, the liquid is not to be suffered to rise above the blackened portion at *d*. If the measures of the different parts be such as have been here given, this cannot take place, for the liquid will fall below the fiftieth division before its other extremity rises above *d*.

The same original volume of gas in $a\ d$ will last for a long time, as we keep replenishing it as often as the fiftieth division is reached.

The experimenter cannot help remarking, that on suddenly exposing the sentient tube to a bright light, *the liquid for an instant rises* on the scale, and on dropping the cap *in an instant falls.* This important phænomenon, which is strikingly seen under the action of an electric spark, I shall consider hereafter.

In conclusion, as to comparing the tithonometric indication at different times, if the gases have the same constitution, the observations will compare; and if they have not the value can from time to time be ascertained by exposure to a lamp of constant intensity. To this method I commonly resort.

From the space occupied in this description the reader might be disposed to infer that the tithonometer is a very complicated instrument and difficult to use. He would form, however, an erroneous opinion. The preliminary adjustment can be made in five minutes, and with it an extensive series of measures obtained. These long details have been entered into that the theory of the instrument may be known, and optical artists construct it without difficulty. Though surprisingly sensitive to the action of the indigo ray, it is as manageable by a careful experimenter as a common differential thermometer.

University of New York, Sept. 26, 1843.

L. *On the Spectral Images of* M. Moser; *a Reply to his Animadversions, &c. By* Robert Hunt, *Secretary to the Royal Cornwall Polytechnic Society* *.

To R. Taylor, Esq.

Dear Sir,

I CANNOT but regret that any remarks which I may have made on the very interesting discoveries of Professor Moser, should have so far disturbed the philosophic quiet of his mind, which it is so important to maintain, when engaged in the investigation of truth, as my paper on Thermography† appears to have done. I am, however, called upon to reply to M. Moser's remarks, which appear in your Journal for November, in a way that is very unpleasing to me. However much men may differ in the interpretations they give to obscure phænomena, I do not fancy they will approach any nearer the truth, or facilitate the progress of inquiry, by indulging in personal attacks. I have ever pursued my inquiries with, I hope, but one object in view. The investigation of curious phænomena has ever been a pleasure to me, and an occasional discovery has been its own exceeding great reward. I never

* Communicated by the Author. † Phil. Mag., Dec. 1842.

expected to be charged with *repeating the experiments of others and giving them out as my own discovery.* The mind of that man, who thinks to elevate himself by any paltry piracy of this kind, is of a low order, and the attempt to defraud the public by any such means is certain, sooner or later, to have its full amount of punishment in the contempt of the many and the pity of the few. But I feel myself put upon my defence. The note you have placed at the foot of page 356 partly relieves me from the charge *, but not entirely; I must therefore presume upon your kindness, and as briefly as possible explain the matter as it stands.

Immediately after the meeting of the British Association at Manchester, I heard, for I was not present at that meeting, of the announcement of M. Moser's discovery, that "when two bodies are sufficiently near, they impress their images upon each other." I immediately tried some experiments, and was much interested in the results. Now, it will be remembered, this announcement at Manchester was unaccompanied by any statement of experiments. I had already made a great number of experiments when I received the *Comptes Rendus* for the 18th of July and the 29th of August, 1842, containing communications, "Sur la formation des images Daguerriennes," which I have distinctly referred to in the very first sentences of my paper on Thermography. These communications gave me M. Moser's views, but not the experimental evidence by which he arrived at these views; and it was not until the publication of the Eleventh Part of the Scientific Memoirs, in February 1843, that I gained any further information on this subject. M. Moser's memoirs appear to have been published in Poggendorff's *Annalen* about June or July 1842, but it is unfortunate for me, that the thoughts and labours of the thinking German nation are sealed books until they appear in my own language, owing to my entire ignorance of theirs. The valuable *Annalen* I have never yet by any chance seen. On the 8th of November I read my paper before the Royal Cornwall Polytechnic Society, the President, Sir Charles Lemon, in the Chair; and this communication, which was immediately printed, and which appeared in many of the leading scientific journals for December, was, I believe, the first series of experiments on this subject published in England.

* Our conviction, upon a comparison of dates, that the charge was groundless, did not deter us from publishing a translation of the paper containing it. On the contrary, we thought it more just towards those included in M. Moser's attack, that they should not remain unaware of misrepresentations which were in circulation abroad, and thus be enabled to meet them.—Edit.

Now, with regard to the experiments themselves, surely Professor Moser will not claim as his own the experiments, of placing a coin on a glass or polished metal plate, or of writing on glass with a piece of steatite, and bringing out the images by breathing on them. Dr. Draper in 1840 published this*; and when a schoolboy, twenty years ago, I tried these experiments without ever suspecting their scientific value, which M. Moser was the first to call attention to. M. Moser, in his memoir 'On the Action of Light on Bodies,' states, "Silver and other metallic plates were *made warm*, and *cold bodies*, variously cut stones, figures of horn, pasteboard, cork, coins, &c. allowed to remain on them for some time." It must be distinctly understood, that at the meeting of the British Association it was stated, that the images could be brought out by the vapours of water, mercury, &c.; but without being, at the period of making my experiments, October 1842, aware of the above, which I did not see until February 1843, my first simple experiments convinced me that some connexion existed between the conducting powers of bodies, as it regards heat, and the strength of the impressions made by them. With this in view I tried good and bad conductors of heat, from copper plates and coins to platina ones, glass and charcoal. These constitute the experiments given in paragraphs from 2 to 7 of my paper. Now if M. Moser used all these materials, and I do not doubt but he may have done so, he certainly did not make his experiments with the same object in view, or he would not have neglected to observe the fact, which I was the first to announce, that "*bodies which are bad conductors of heat placed on good conductors make decidedly the strongest impressions.*" I am quite ready to give up any claim to the experiments, but I reserve to myself the interpretation they afford.

M. Moser says, "I cannot name a single experiment, &c. &c. which I had not previously described." Will M. Moser oblige by directing me to any of his memoirs, where may be found the experiments named in paragraph 8, which show the power of electrical discharges in evoking again these mysterious images after they have been effaced? Or that in paragraph 15, where a copper plate is described to have been so changed in its molecular constitution, by being warmed in contact with a piece of paper, that it readily amalgamated with mercury over the parts which the paper covered, but not so over the other portions of the plate?

Nearly all the other paragraphs of my paper, to the 22nd, are details of experiments with coloured glasses and transparent bodies, placed upon plates of unprepared copper and

* In Phil. Mag., S. 3, vol. xvii. p. 217.—Edit.

silver. I find that M. Moser has also used coloured glasses, but principally upon iodized silver plates. It is to be lamented that he has made so great a number of very careful experiments in this way; for, by regarding the colour of the glass, and the colour of the ray which permeates it, as the same, he has been led to some very incorrect conclusions, as the slightest acquaintance with the valuable labours of Sir John Herschel would have shown him. My use of coloured glasses in *these experiments* was confined to the *heating powers of the different colours*, and these were contrasted with smoked glasses, and the like, the results showing, whether the experiments were made in sunshine or at night, that those glasses, the red and blackened ones, which admitted the permeation, or absorbed the largest quantity of *heat*, made the most decided impressions on metal plates. I cannot see how M. Moser makes out his claim to these experiments, except it is upon the principle that Professors Faraday and Daniell are guilty of scientific piracy in publishing, in their valuable memoirs, results obtained with zinc and copper plates, Volta having used the same kind of plates before them.

I have only to deal with one more of Professor Moser's charges. He says, "He has not devised a single new experiment, for even those which appear to him sufficiently important to be adopted as the running head of his paper, 'The art of copying engravings, or any printed characters from paper on metal plates,' will be found nearly word for word in the *Annalen*, vol. lvii. p. 570." The latest memoir of M. Moser with which I am acquainted, is the 18th article in the 3rd vol. of the Scientific Memoirs, which is stated to be "from Poggendorff's *Annalen*, Band lvii., 1842, No. 9. p. 1." I presume M. Moser alludes to a more recent publication. He, however, relieves me from a difficulty by saying, "*It is that experiment in which I caused a* SEAL *to depict itself on mercury with which a pure or silvered copper-plate had been coated, and afterwards produced the image in the iodine vapours.*" Now we will examine the similitude between this and my published experiment. A copper plate is amalgamated by nitrate of mercury, and a *line or mezzotinto engraving, a wood-cut or lithographed print*, ON PAPER, is placed upon it for a few hours. With certain precautions the plate is exposed to the *vapour of mercury*; this vapour attacks those parts of the plate which correspond with the white parts of the paper, and a faint image is formed; the plate is now placed in the *iodine box* for a little time, and its vapour attacking and blackening those parts of the plate, which correspond with the dark portions of the paper, brings out a very decided and beautiful copy of the print. I am quite satisfied to leave it to yourselves and your readers to say if this "sufficiently important" experiment is

M. Moser's, or otherwise. I have seen paragraphs stating that M. Moser has succeeded in copying engravings from paper, but I do not, even now, know his experiments. I shall not dispute with M. Moser the point of priority, but I trust he will do me the justice of acknowledging, that he has judged hastily in accusing me of having appropriated his experiments without acknowledgement. In the paper in question I thought I had sufficiently acknowledged the high merits of Professor Moser; I again do so. He has opened a new and important path of physical inquiry, which promises to lead to some great truths connected with the constitution of matter, and the operations of the imponderable elements; at the same time, however, that I admit the importance of his discoveries, I must be allowed, for the present, to dissent from his conclusions.

It is not my intention to offer any further remarks in explanation of that portion of M. Moser's paper which particularly applies to myself, but I must be allowed this opportunity of reviewing some of the opinions he has put forth, and of explaining my reasons for differing from him.

M. Moser states *that every body must be considered as self-luminous*, and he appears to view the accelerating power exerted by heat, as stated in his own experiments, as the influence of caloric increasing the intensity of the invisible radiations, whilst as "their temperature becomes higher their refrangibility decreases." It becomes necessary, in the first place, to ascertain upon what evidence this *self-luminosity* of bodies is asserted. It has been long known, that light acting upon ioduret of silver, alters its condition, and renders it capable of condensing the vapours of mercury in a remarkable manner;—this constitutes the Daguerreotype. It has been shown that if we breathe over portions of a metallic plate, the other parts being covered, and then the vapour is allowed to dry off, the plate is in a condition to receive vapours over definite spaces, in the same manner as if it had been exposed to the light. Again, any solid body being placed for a short time on a polished plate of metal or of glass, either of them become susceptible of receiving vapory deposits, which will mark distinctly the spaces occupied by the bodies in contact. "By these experiments, I think," says Moser, "*I have proved that contact, condensation of vapours, and light produce the same effect on all bodies*;" and hence he rushes to the conclusion that all bodies are self-luminous, and has even speculated on the colour of the latent light of vapours, in a way, which betrays his fears lest the hypothesis he has framed should be destroyed by his own results. Light, or rather, as I am inclined to think, some element intimately connected with light, and

having its origin in the sun, but broadly distinguished from it by its producing no influence on the organs of sight, certainly "so modifies the surfaces of bodies that they condense vapours otherwise than usual;" and like modifications are produced by condensing vapours on parts of the surface, or by placing other bodies in contact with it. Whatever method we may adopt to disturb the surface of any body, be it metallic or vitreous, we have an unequal condensation of vapour. If without touching the surface of a metal plate, we subject it to the disturbance produced by a blow or two on the back of the plate, we shall find an irregular deposit of vapour if we breathe on it. The molecular change which bodies undergo, under the most trifling circumstances, is certainly one of the most curious matters with which photography has brought us acquainted. By light, by heat, by electricity we can dispose plates to receive vapours over definite spaces; by lowering the temperature of parts of any body, the same effect is produced, and by any mechanical force we do the same. If we place a copper plate so that one half of it shall *rest on* a cold body, and apply the heat of a spirit lamp for a few seconds to the other half, carefully avoiding touching the polished surface, and then expose the plate, when *quite cold*, to mercurial vapour, it will be found that a larger quantity of vapour is deposited over the half that was warmed than over the other half.

If a piece of wood is placed upon a polished plate, and one or two gentle blows is given to it, the plate will exhibit, when submitted to vapour, not merely the shape of the wood, but a perfect picture of its fibres. Again, if we give a metal plate a few gentle blows upon the *back*, the *surface* will distinctly show, when exposed to vapour, the spaces corresponding with those on the back on which the hammer fell. If we subject portions of a metal plate to any chemical action, even though it may be inappreciable to the sight, it will exhibit the spaces to which the action was confined, the moment it is exposed to the influence of vapour.

These experiments afford us a sufficient amount of evidence to conclude, *that any cause producing a change upon solid surfaces disposes them to condense vapours unequally.* They also prove the correctness of all the statements made by M. Fizeau, Professor Grove, Mr. Prater * and others; and at the same time as they do this, they convincingly show us, that these observers have only been dealing with a few curious facts, which cannot be allowed to explain these remarkable phænomena, to which, in particular, M. Moser wishes to direct attention, viz. the power which the solar rays have of produ-

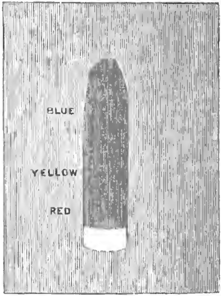
* See p. 225 of the present volume.—Edit.

cing definite changes in the condition of the surface of solid bodies, and the remarkable property of two substances in juxtaposition, inducing upon each other changes which may be rendered evident to the senses; and these changes, it must be remembered, are not confined to the surface merely, but they penetrate to a considerable depth into the solid structure of the mass, as I have already proved in several of my published experiments.

These results, in addition to many which I have previously given, also show the impropriety of considering these spectral images as the effects of "*invisible light*," "*light radiated in absolute darkness*," as they have been by M. Moser. I must in this place express my conviction, that any term involving an idea contrary to our received ideas, is calculated to produce much confusion. Light is that element which affects the organ of sight, enabling us to distinguish objects; that which does not do this is no longer light. The prismatic spectrum has been proved to consist of one element giving *light* and *colour*, of another element affording *heat*, and of a third element, which is active in producing chemical change. Now M. Moser informs us that it is neither of these, but a class of rays which are still more refrangible than those which have been called the "invisible chemical rays." "According to my experiments," he says, "the invisible rays of light pass very readily through aqueous solutions of various kinds, and through different oils, but they decidedly do not pass through the thinnest plates of glass, mica, or rock-salt, &c." Now, as M. Moser has not given us his experiments, it is very difficult to deal with them. In the first place, he has not, as it appears to me, as yet afforded any evidence of the existence of such a class of rays as those he speaks of; and if the "thinnest plates of glass, &c." are not permeated by these rays, upon what principle does he, by the use of *coloured glasses*, prove "that light and mercurial vapour are identical in their effects?" The prism must be useless in this inquiry on M. Moser's own showing; I am therefore quite at a loss to know by what means he has succeeded in establishing, with so much accuracy, the refrangibility of these "invisible rays of light." I am very far from denying that the phænomena in question have been produced by *invisible solar emanations*. I am not at all prepared to deny the absorption of such emanations by solid bodies, or their existence in a latent state, or their radiation in darkness. I contend only that LIGHT has nothing whatever to do with the phænomena. In the present state of the inquiry it does appear to me that HEAT is a very active agent in producing the effects in question; indeed M. Moser himself says "if the temperature be raised, they (the invisible

rays of light) pass very readily plates of glass and mica." I shall have to name an experiment or two presently which will show, in a striking manner, the influence of the "calorific rays" on metallic plates. I regard those emanations, which are the acting ones in all our photographic operations, as possessing powers of the most energetic and extraordinary kinds; powers which are in constant activity, decomposing and recomposing, maintaining the conditions of growth and decay, of vitality and corruption; indeed, effecting that mighty system of change, which is the order of the visible creation, and these *may be* the radiations by which all the spectral images of Moser are produced. Sir John Herschel* was inclined to attribute these phænomena to a class of rays ranging above the red rays of the spectrum, and to which he gave the name of "parathermic rays." The evidence, however, of the existence of these rays, as a distinct class, is not sufficiently clear even to satisfy the mind of this talented and most indefatigable observer, to whom we are indebted, more than to any other person, for our knowledge of the conditions of the solar spectrum.

At the Cork meeting of the British Association I communicated the results of some experiments made with the prismatic spectrum itself, which is the only way in which, as far as I am aware, we can arrive at any satisfactory determination on the point in question.



A condensed prismatic spectrum was, by means of a good heliostat, maintained in one place upon a very highly polished copper plate for three hours. At the expiration of that time, the plate was exposed to the action of mercurial vapour. This vapour was condensed over the whole plate, but in very different proportions beyond and within the limits of the spectrum. The space occupied by the luminous image was evidently protected from that influence which disposed the other parts of the plate to condense the

* Philosophical Magazine, July 1843, (pres. vol.) p. 510.

vapour, but the space occupied by the extra spectral red rays, had undergone that change, which renders metals most susceptible to the action of vapours, and a thick white line of vapour very distinctly marked this calorific region, giving a spectral image similar to the accompanying figure. It does not appear that so long an exposure to solar influence was necessary, for a similarly condensed spectrum was allowed to *traverse over* a polished copper plate for a few hours. On submitting this plate to vaporization, a line of thickly-deposited mercurial vapour distinctly marked the path of the extra-spectral red rays. I should explain that I mean the extreme red ray of the spectrum, which is seen when it is looked at through a cobalt-blue glass, and which has been made the subject of much attention by Sir John Herschel, and some rays below this extreme red ray. These rays cover a space which correspond as nearly as possible with the large heat-spot in Sir John Herschel's thermographic spectrum. I have been exceedingly anxious to repeat these experiments on other metals, but the unfavourable state of the weather, and the advanced season has compelled me to abandon this examination for the present.

M. Moser has stated that this "invisible light" will not permeate glass. In my first communication on "Thermography" (Phil. Mag. S. 3., vol. xxi. Dec. 1842, p. 464), paragraph 9, I have shown that some influence is exerted through red glasses which is not exerted through blue glasses. It may be said that this was an influence radiated from the red glass, in greater quantity than from the blue. I think, however, that I have distinct proof of the permeation of the rays which are active in producing these spectral images.

Three very large flat white glass bottles were provided, and filled with coloured fluids, blue, yellow and red. The light had to pass through about $1\frac{1}{4}$ inch of fluid in each case, this was very carefully examined with the prism, and the depth of colour adjusted until they represented, very fairly, the three great divisions of the spectrum. Figures were cut in paper and placed upon highly polished copper plates, being pressed down by these bottles of coloured fluid. This arrangement being left in the dark during a night, the plates were submitted to vapour, and they exhibited, each of them, impressions of the paper figures, in which little or no difference could be detected. The same arrangement was exposed for different periods, varying from half an hour to two hours, to the influence of the sun's rays. Under the bottle containing the red fluid, a most perfect image was formed by mercurial vapour, even after the shortest exposure. Prolonged exposure gave a

faint image on the plate under the yellow fluid, but no trace of an impression could be detected, by the influence of the same exposure, under the blue fluid. Surely these results prove, that the calorific rays are the most active in these phænomena; and instead of inventing the purely conjectural notion of "invisible light," is it not much more rational, when we have distinct evidence of the powerful action of heat, to look for an explanation of these phænomena in the calorific radiations, which are equally active in light or darkness? Instead of assuming that bodies are all *self-luminous* for the purpose of explaining these curious facts, which "self-luminosity" we are not in a condition to prove, is it not more consistent with the spirit of inductive philosophy, to seek for the cause in the *invisible radiations of* HEAT, which we know take place under all circumstances? And it is admitted, even by Moser himself, that heat is a powerful accelerating agent.

M. Moser has not told us how he has determined that his *dark light* passes readily through aqueous solutions and oils. I have tried some experiments which are instructive, and I therefore record them. Having put an edge of wax around a polished copper plate, to the depth of one-eighth of an inch, I covered the plate with water, and upon two small pieces of glass I supported, by the edges, a silver medal, so that it just touched the upper surface of the water; in twelve hours the plate was much tarnished over every part, except that directly under the medal, which remained as bright as at first. Under one of the pieces of glass a very decided change of colour was produced, and from its whiteness I was at first inclined to think, that silver had been removed from the medal, and deposited on the plate; I have, however, since proved that it was an oxidation of the copper plate merely. The same arrangement was made with very fine olive oil, and the result was similar; but upon leaving the medal and plate undisturbed for some days, the whole surface of the copper was oxidized, the oil became a very fine green colour, and the under surface of the silver was covered with a film of copper. Here we have analogous phænomena to those we have been considering, but in these cases it is very evident the cause was neither light nor heat, but *voltaic electricity*. The plates, after the water and oil were removed, were thoroughly cleaned and exposed to the vapour of mercury, which gave images of the medal in outline, and of the glasses.

It may not be out of place here to say a few words relative to the explanation given by M. Fizeau of the production of these images. There is no doubt "if different parts of a polished surface are unequally soiled by extraneous bodies, even

in an exceedingly minute quantity," that it will condense vapours irregularly. But in the communication which I made to the British Association at Cork, I stated several experiments, which appear to me to prove that the images are produced quite independently of any layer of organic matter. Copper plates were polished with water only and boiled, and all the things placed on these plates were boiled also, yet very perfect images were produced. I have since tried the effect of exposing the plates, &c. separately to a very strong heat, and when cold placing them in contact; there has been no apparent difference between the images formed under these circumstances, and those formed upon plates, and with medals, &c. which have been purposely covered with very slight films of organic matter. I have also repeated these experiments many times, in the best vacuum which could be maintained with a good air-pump, to avoid the action of any vapours on the metal or glass plates, and in every case the images have been well defined. I cannot imagine any volatile film of the kind described, exerting an influence to so great a depth into the solid metals as I find to be the case. Often I have, by polishing, removed layers of metal, and yet the images have been reproduced by exposing the plate to vapour; and in paragraph 8 of my paper on Thermography, above alluded to, I have stated the result of an experiment in which electricity reproduced a succession of images which had been obliterated from the copper plate.

It has been suggested that electricity may be engaged in the production of these spectral figures. I have just tried an experiment which appears to show the probability of this element's being involved in some way in these very complicated phænomena. I arranged four electro-positive metals, nickel, bismuth, cadmium and silver, and two electro-negative ones, arsenic and antimony, on a copper plate, and they were allowed to rest upon it for three hours. Being removed, the plate was submitted to the vapour of mercury. The space covered by the nickel was marked, by being left free of vapour; that on which the cadmium lay was still more decidedly marked in this way; where the bismuth was placed the image was exceedingly faint, but still it was observable by a deficiency of vapour; and the silver was more decidedly outlined with vapour, but none on the spot it covered. On the contrary, the space occupied by the antimony was *covered in a most remarkable manner with vapour, presenting a perfectly white spot*, which in all positions distinguished it from the other parts of the plate, whilst the arsenic left no trace behind.

I think I have now shown, that many different causes may

produce similar effects, and certainly I have established the necessity of examining all the phænomena with great care and attention, before we attempt to establish a theory which shall embrace the whole class. I have given the name Thermography to these images, under a conviction that heat was most importantly engaged in producing them, and I see no reason for altering the name. At the same time I beg to be very distinctly understood, that I am not wedded to this opinion; I feel conscious that we are dealing with some of the most subtile agents in nature, and that we cannot too jealously guard against the deceptions of the senses. That which heat appears to produce, may be the creation of some other element, which is excited only by calorific influence. But although I hold my judgement under suspense, particularly when I find light, heat, electricity, chemical action and mechanical force, all producing the same effects, I cannot at present entertain the idea of "invisible light," although M. Moser states, that in his memoir on Vision, he has demonstrated its existence. In conclusion, I must hope that I have been successful in proving that I have not in this instance, or in any other, endeavoured to appropriate the experiments of another. I have ever studiously endeavoured to give the merits of even the slightest suggestion to its author, and if in any one instance, in the case of Professor Moser, I have not done so, I have erred through ignorance, and not by design. I cannot, however, detect any grounds for M. Moser's attack. I commenced my first paper with a statement of his results, and I concluded it by giving my opinion on the importance of the discoveries he had made. I cannot, however, allow myself to be led away from that which appears to me to be the legitimate path of inquiry by any unkind feeling. I shall pursue the investigation, and the moment I can convince myself that light is engaged in these phænomena in darkness, I will acknowledge the correctness of Professor Moser's views with heartfelt pleasure.

Falmouth, November 4, 1843. ROBERT HUNT.

LI. *On Theine and its Preparation.* By JOHN STENHOUSE, *Esq., Ph.D.**

THE process which I have found most suitable for preparing theine, is both easy and productive, and is simply as follows:—A decoction of tea is first treated with a slight excess of acetate of lead, which throws down the tannin, and almost

* Communicated by the Chemical Society, having been read March 21 and May 2, 1843.

all the colouring matters it contains. It is filtered while hot, and the clear liquor is evaporated to dryness. It forms a dark yellowish mass, which is to be intimately mixed with a quantity of sand, and introduced into Dr. Mohr's subliming apparatus. This should then be set upon a sand-bath, or still better on a metallic bath, and a moderate heat applied to it for 10 or 12 hours. The theine sublimes in beautifully white, anhydrous crystals, and is deposited upon the paper diaphragm which runs across the apparatus. The only thing to be observed is, that the temperature should never rise too high, as the more slowly the operation is conducted, the finer are the crystals and the greater is their quantity. The following are the results obtained by this process in four different trials.

I. One pound of green Hyson tea gave 72 grains of perfectly white theine, and 2 grains which were slightly coloured, in all 74 grains = 1·05 per cent.

II. 8 oz. of black Congo tea gave 34·5 grains pure theine, and 1·5 grain impure, in all 36 grains = 1·02 per cent.

III. 6 oz. black Assam tea yielded 36 grains theine = 1·37 per cent.

IV. 1 lb. of a cheap green tea called Twankay, gave 69 grains = 0·98 per cent. This last was sublimed too quickly, or it would probably have given a little more theine.

We have four determinations of the theine in different specimens of tea by Mûlder. He found in Chinese Hyson 0·43 per cent. theine, in congo 0·46 per cent, in Japanese Hyson 0·60 per cent., and in Japanese Congo 0·65 per cent.

Mûlder's process consisted in boiling the filtered decoction of the tea with magnesia, evaporating to dryness and then dissolving out the theine from the dry mass with æther. I have repeatedly tried his process, but found it very troublesome and unproductive; besides, the theine always requires more than one crystallization to render it quite pure, and the high price of æther, in Great Britain, at least, is a very serious inconvenience. It is to the presence of theine, I believe, that the bitter taste of tea is chiefly owing, and a tolerably correct idea of the quantity of theine in any specimen of tea may be formed from the degree of bitterness which it exhibits. The Assam tea, which yielded such a large proportion of theine, was remarkably bitter, but it appeared rather deficient in essential oil.

Theine may also easily be made from coffee, by a slight alteration of the method just described; the coffee beans should not be roasted, as this would drive off much of the theine, but only slightly dried, and then ground or pounded, and repeatedly boiled with water till exhausted. The filtered

decoction should first be precipitated while hot with basic acetate of lead. It should then be filtered and boiled with a little hydrated oxide of lead, which occasions a further precipitate, which is also to be separated by filtration. The clear liquor is to be evaporated to dryness, and sublimed exactly in the same way as the extract of tea. From a pound of coffee, in the course of several trials, I obtained from 12 to 18 grains of theine, which was sometimes not so perfectly white as that made from tea, as it was accompanied by a little more empyreumatic oil. It was easily rendered perfectly pure with scarcely any loss, by subliming it a second time at a very moderate temperature. A considerable quantity of theine may be easily made by sublimation, from either tea or coffee, in the course of two days, while the ordinary way of procuring it is both tedious and expensive.

Several chemists have been induced to affirm that none of the beneficial effects which tea and coffee produced on the animal œconomy, should be ascribed to the theine they contain, owing to the smallness of the quantity in which they supposed it to exist in these substances. Professor Liebig has, as is well known, recently advanced the very opposite opinion, and has rendered it highly probable that theine will yet be found to possess valuable medical properties. I hope that some medical practitioners may soon be induced to try if its utility in medicine will be found equal to the expectations which have been formed of it.

In reference to the sublimation of theine, I may mention that I have found it advantageous to make a slight addition to Dr. Mohr's subliming apparatus, described in a previous paper. Instead of pasting the diaphragm of bibulous paper immediately on the rim of the iron pan, I paste it on a moveable rim of tin plate about an inch deep, which goes close round the outside of the pan, and which projects about one-eighth of an inch within it. When covered with the paper it exactly resembles a small sieve, and may be easily removed and replaced at pleasure. This enables us to stir the mass we may be subliming from time to time, and thus to heat the whole of it more equally. Now this could not be done in the usual apparatus without destroying the diaphragm.

Theine in Paraguay Tea.—I am indebted to the kindness of Professor Christison for a quantity of Paraguay tea, or "yerba mate," as it is called. It consists of the leaves and small branches of the *Ilex paraguayensis*, which after being strongly roasted, have been reduced to a coarse powder. This substance is extensively used in South America as a substitute for tea. Its taste is very bitter, partly resembling

that of ordinary tea, but also approaching somewhat to that of sumach. Its reactions were studied a number of years ago by Professor Tromsdorff, and are pretty fully stated in the eighteenth volume of the *Annalen der Pharmacie*, page 90. As my own observations coincide pretty closely with his, it appears unnecessary to give a detailed account of them. In proceeding to examine Paraguay tea for theine, acetate of lead was added to its decoction, which threw down a very dense greenish-yellow precipitate, and when this was removed by filtration, subacetate of lead also produced a considerable quantity of a bright yellow precipitate. The clear liquid when drawn off and evaporated to dryness, left a good deal of a tenacious dark brown mass, which was very hygroscopic. When a little of it was subjected to distillation, a quantity of long flat crystals, exactly resembling the theine, sublimed into the sides and neck of the retort; and at the same time the very peculiar pungent smell which theine always emits when subliming became very perceptible.

The remainder of the brownish-yellow substances already mentioned was reduced to fine powder, and intimately mixed with a large quantity of sand to prevent its agglutinating. It was then repeatedly agitated with æther in a stoppered bottle. The æthereal solution when poured off and distilled pretty low, deposited a quantity of crystals which were slightly coloured at first, but which were rendered perfectly white by repeated crystallizations. In their crystalline form, taste, solubility in water, alcohol and æther, and in all their reactions, they correspond exactly with ordinary theine. I may mention also, as an additional confirmation of the truth of this opinion, that in the course of some experiments upon theine, I have found an excellent test for that substance, by which its presence even in small quantities may be readily detected. It consists in the action of nitric acid upon theine, the effects of which are very different according to the quantity of the acid employed, and the length of time during which its action is continued. If theine is boiled for a few minutes with only twice or thrice its weight of fuming nitric acid, nitrous gas is given off in abundance, and a bright yellow solution is obtained. If a little of this liquid is taken out and gently evaporated to dryness in a porcelain basin, it leaves a deep yellow mass. If a drop of ammonia is then let fall upon it and a gentle heat is applied, a bright purple colour is immediately produced, which cannot be distinguished by the eye from that obtained from uric acid when similarly heated. This rich purple colour is permanent, its aqueous solution has a deep crimson shade. It also dissolves in

spirits of wine, but it is not soluble in æther. Its colour is immediately destroyed by a solution of potash, which does not change it to an indigo-blue, as it does murexide. The substance which gives the red colour with ammonia does not appear to be crystallizable.

Now as I obtained the purple colour as readily from theine prepared from Paraguay tea as from that made from ordinary tea and coffee, I have not the least doubt that they are both identical substances. Unfortunately, from the smallness of the quantity of Paraguay tea in my possession, I have not been able to procure more than a few grains of the theine in a state of purity, and have been prevented therefore for the present from subjecting it to analysis. This however I have good reason to believe I shall be able to do in the course of a few weeks.

The quantity of theine in Paraguay tea is by no means great, but no doubt much of what it originally contained had been destroyed by the injudicious way in which it is manufactured in Paraguay. The branches of the yerba tree are there cut down and spread upon a sort of wooden barbacue, under which large fires are kept burning. The yerba is therefore exposed to a very high temperature, and as theine sublimes pretty readily, it is plain that a good deal of it will necessarily be dissipated.

It is somewhat singular, as Professor Liebig has observed, that the other three vegetable substances which are known to contain theine, tea, coffee, and guarana, though derived from plants of very different natural families, are all of them extensively employed as refreshing beverages. The circumstance that Paraguay tea, which is extensively applied to precisely the same purpose, also contains theine, is calculated, I should think, to give additional probability to the views of Professor Liebig on this subject.

It is not unlikely that theine will soon be found to occur in other vegetables besides those in which it is already known. The easiest way perhaps to examine a plant for theine, which can be done in the course of a few hours, is to precipitate its infusion with subacetate of lead, to filter and evaporate the clear liquid to dryness. If a portion of the matter thus obtained be distilled, any theine it may contain will be immediately deposited in long flat crystals on the neck and sides of the retort.

Camellia Japonica.—Through the kindness of Professor Balfour I was enabled to examine a quantity of the leaves of the *Camellia Japonica*, a plant whose botanical characters approach very closely those of Tea Bohea. I found that the

Camellia does not contain any theine, and indeed its chemical properties have very little resemblance to those of the tea plant. It has scarcely any of the bitter astringent taste by which both green and black tea are distinguished, and it appears wholly devoid of any essential oil. It contains, however, a small quantity of tannin, which gives olive-green precipitates with salts of iron, and very copious yellow precipitates with acetate of lead. It occasions a very slight precipitate only in a solution of gelatine, and does not precipitate tartar-emetic at all.

Besides tannin the Camellia also contains a quantity of mucilage, some chlorophyle, and a waxy resinous matter.

I have also examined the holly, the *Ilex Aquifolium*, for theine, but without success. Its chemical properties appeared pretty similar to those of the *Camellia Japonica*.

Action of Nitric Acid upon Theine.—As has been already mentioned, when theine is boiled with three or four times its weight of strong nitric acid, it is converted with copious evolution of nitrous gas into a deep yellow liquid, which when gently evaporated to dryness and slightly warmed, gives with ammonia a purple colour similar to that of murexide. This we have also mentioned is an excellent test for theine, and it forms a very pretty class experiment. The yellow liquid contains a very soluble crystalline body, which, when most of the nitric acid has been driven off, and the solution evaporated nearly to a syrup, crystallizes in long, hard, colourless needles. They have a rather sweetish taste, and when freed from adhering acid by repeated crystallizations, appear to be neutral to test paper, or at least only very slightly acid. Both this and the red colouring matter, however, appear to be the products of the imperfect oxidation of the theine. If the theine is boiled for some hours in a great excess of nitric acid till a drop of the solution, when evaporated to dryness, is no longer yellow but white, the addition of ammonia does not produce any change of colour whatever. Both the yellow liquid and the substance which crystallizes in needles are then found to have disappeared. If the greater portion of the nitric acid is distilled off, and the liquor concentrated to a syrup as before, it readily concretes on cooling into a mass, containing a number of large shining crystals. The mother-liquor which surrounds them appears to consist chiefly of very deliquescent ammoniacal salts. The crystals have a sweetish taste, grate between the teeth, and have a bright silvery lustre. Their crystalline form is not at all distinct, but they form large plates which readily crystallize. They dissolve in about three times their weight of cold water, but in a much smaller portion of

hot water. They are readily soluble also in alcohol and æther. Their great solubility renders it a little difficult to purify them. The best way of doing so is by repeatedly crystallizing them out of water, and by pressing them between the folds of blotting-paper. When purified they are neutral, or at least redden litmus paper very feebly. The smallest portion of an alkali when added to their solution renders it alkaline. When heated they readily sublime, and are deposited in fine shining crystals on any cold object. They take fire easily and burn with a bright flame. They do not give off ammonia when heated with potash.

They occasion no precipitate or change of colour in solutions of nitrate of silver, acetate of lead, or sulphate of iron.

I hope soon to subjoin the results of their analysis, with a more minute account of their properties.

5th. Two lbs. of coarse black bohea tea, such as usually sells for three shillings a pound, gave 99·5 grs. theine = 0·70 per cent.

Analysis of the sublimed Theine.—0·285 substance gave 0·5125 carbonic acid, and 0·132 water =

		Calculated Number.
Carbon ...	49·72	49·79
Hydrogen	5·14	5·08

Analysis of Theine from Paraguay Tea.—0·2905 dried at 212° gave 0·525 carbonic acid, and 0·1345 water =

		Calculated Number.
Carbon ...	49·96	49·79
Hydrogen	5·145	5·08

The want of material unfortunately prevented me from determining the nitrogen also.

Hydrochlorate of theine forms a double salt with chloride of platinum. It may be easily obtained in small but very distinct orange-coloured crystals by adding chloride of platinum to a hot solution of theine in hydrochloric acid. In a few minutes as the liquor cools the crystals begin to form, and gradually subside as a mass to the bottom of the vessel. When dried at 212° and burnt,

				Per cent.	Calculated Number.
I.	0·461 grs. salt gave	0·112	of platinum	= 24·29	... 24·48
II.	0·529 ...	0·130	...	= 24·57	
III.	0·5828 ...	0·143	...	= 24·53	
IV.	0·458 ...	0·112	...	= 24·45	

These numbers lead to the rational formula $C^{16} H^{10} N^4 O^4 H Cl + Pt Cl^2$, which gives 24·48 per cent. platinum, and double the ordinary atomic weight of theine.

Atoms.			Per cent.
16 C	= 1222·960		
10 H	= 124·795		
4 N	= 708·160		
4 O	= 400·000		
1 H Cl	= 455·140		
2 Cl	= 885·300		
1 Pl	= 1233·26	=	24·48
	5029·615		

The salt employed for the first two determinations was washed with alcohol, and that for the last two with æther. This double salt appears therefore much more stable than the hydrochlorate of theine. I am proceeding to determine the quantity of its other constituents.

Complete Analysis of the Theine prepared by Sublimation.*

0·285 gr. of the substance gave 0·5125 carbonic acid and 0·132 water. When burnt with oxide of copper, eight tubes gave carbonic acid and nitrogen in the proportion of four to one.

	Found.	At.	Calculated numbers.
Carbon	49·72	16 Carbon	=49·798
Hydrogen	5·14	10 Hydrogen	= 5·082
Nitrogen	28·78	2 Nitrogen	=28·832
Oxygen	16·36	4 Oxygen	=16·288
	100·00		100·000

Theine from Paraguay Tea.

Through the kindness of my friend Professor Gardner, I have procured an additional quantity of Paraguay tea, which has enabled me to complete the analysis of the theine it contains, and also to determine its quantity. The easiest and most œconomical way of obtaining theine from Paraguay tea is by sublimation. The filtered infusion of the tea, after being treated with acetate of lead and the precipitate removed, should be boiled with an excess of litharge, the clear liquid evaporated to dryness and sublimed with the usual precautions. One quantity of two pounds yielded 12·5 grs. of anhydrous theine, and a second quantity of equal amount, which was more successfully treated, gave 14·5 grs. = 0·13 per cent. This is about half the quantity which most kinds of coffee yield, which I have found to vary from 12 to 18 grs. for a pound, and about ten times less than Chinese tea, a pound of which yields from 70 to 90 grs.

* The portion of this paper read May 2, begins here.

Analysis of Theine from Paraguay Tea.

I. 0·2095 gr. of the substance dried at 212° gave 0·525 carbonic acid and 0·134 water.

II. 0·3192 gr. gave 0·572 carbonic acid and 0·1487 water.

When burnt with oxide of copper, nine tubes gave carbonic acid and nitrogen in the proportion of four to one.

	Found. I.	II.	At.	Calculated numbers.
Carbon	49·960	49·54	16 Carbon	=49·798
Hydrogen	5·145	5·17	10 Hydrogen	= 5·082
Nitrogen	28·927	28·68	2 Nitrogen	=28·832
Oxygen	15·968	16·61	4 Oxygen	=16·288
	100·000	100·00		100·000

The following is a more complete analysis of the double salt of muriate of theine and chloride of platinum:—

I. 0·4637 salt gave 0·412 carbonic acid and 0·1155 water.

II. 0·4347 gave 0·386 carbonic acid and 0·1184 water.

I. 0·461 salt gave 0·112 plat.=24·29 per cent.

II. 0·529 salt gave 0·130 plat.=24·57.

III. 0·5828 salt gave 0·143 plat.=24·53.

IV. 0·458 salt gave 0·112 plat.=24·45.

	I.	II.	At.	Calculated numbers	per cent.
C	24·56	24·55	16 C	= 1222·9	24·32
H	2·76	3·02	11 H	= 137·2	2·72
			4 N	= 708·1	14·08
			4 O	= 400·0	7·96
			3 Cl	= 1327·9	26·41
Pt	24·46	24·46	Pt	1233·2	24·51
				5029·3	100·00

The rational formula for the salt is $C_{16}\,H_{11}\,N_4\,O_4\,Cl\,H +$ Pt 2 Cl. The formula for Theine,—

16 Carbon	=1222·96
11 Hydrogen	= 124·79
4 Nitrogen	= 708·16
4 Oxygen	= 400·00
Atomic weight	=2455·91

Nitro-theine.

I have given the provisional name of nitro-theine to the substance crystallizing in large shining plates, produced by the long-continued action of an excess of boiling nitric acid upon theine. It is not necessary to employ fuming nitric acid to obtain this substance, acid of the ordinary strength answers equally well. Nitro-theine when crystallized out of water forms large shining plates resembling cetine, but having more

of a pearly lustre. When sublimed, its crystals resemble those of naphthaline, and when formed by spontaneous evaporation from æther they are deposited in large, very regular rhombohedrons. I formerly stated that alkalies do not evolve ammonia from nitro-theine; I find that in this I was mistaken, for when boiled with solution of potash it gives off abundance of ammonia.

When subjected to analysis,—

I. 0·2628 gr. of the substance dried at 212° gave 0·398 carbonic acid and 0·1005 of the water.

II. 0·2529 gr. gave 0·3855 carbonic acid and 0·0975 water.

When burnt with oxide of copper, ten tubes gave carbonic acid and nitrogen in the proportion of five to one.

	I.	II.
Carbon	41·87	42·15
Hydrogen	4·24	4·28
Nitrogen	19·39	19·56
Oxygen	34·50	34·01
	100·00	100·00

Nitro-theine appears to be a neutral body, and as I have not been able to determine its atomic weight, I have not thought it worth while to attempt to deduce any formula from these analyses. Theine does not yield more of this substance than from 5 to 6 per cent.

LII. *On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat.* By J. P. Joule, *Esq.*

[Continued from p. 355 and concluded.]

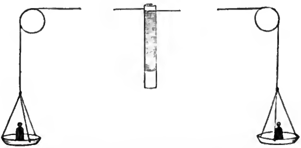
Part II.—*On the Mechanical Value of Heat.*

Having proved that *heat* is *generated* by the magneto-electrical machine, and that by means of the inductive power of magnetism we can *destroy* or *increase* at pleasure the *heat* due to chemical changes, it became an object of great interest to inquire whether a constant ratio existed between it and the mechanical power gained or lost. For this purpose it was only necessary to repeat some of the previous experiments, and to ascertain, at the same time, the mechanical force necessary in order to turn the apparatus.

To accomplish the latter purpose, I resorted to a very simple device, yet one peculiarly free from error. The axle *b*, fig. 1, (p. 264) was wound with a double strand of fine twine, and the strings (as represented in fig. 8) were carried over very easily-working pullies, placed on opposite sides of the axle, at a distance from each other of about 30 yards. By means of weights placed in the scales attached to the ends of the strings, I could

easily ascertain the force necessary to move the apparatus at any given velocity; for, having given in the first instance the required velocity with the hand, it was easily observed, in the course of about 40 revolutions of the axle, corresponding to

Fig. 8.



about 270 revolutions of the revolving piece, whether the weights placed in the scales were just able to maintain that velocity.

The experiments selected for repetition first were those of series No. 2. Ten cells, in a series of five double pairs, were connected with the large electro-magnet; and the small compound electro-magnet (restored to its place in the centre of the revolving tube) was connected, through the commutator, with the galvanometer. Under these circumstances a velocity of 600 revolutions per minute was found to produce a steady deflection of the needle to 24° 15′, indicating 0·983 of current magneto-electricity.

To maintain the velocity of 600 per minute, 5 lbs. 3 oz. had to be placed in each scale; but when the battery was thrown out of communication with the electro-magnet, and the motion was opposed solely by friction and the resistance of the air, only 2 lbs. 13 oz. were required for the same purpose. The difference, 2 lbs. 6 oz., represents the force spent during the connexion of the battery with the electro-magnet in overcoming magnetic attractions and repulsions. The perpendicular descent of the weights was at the rate of 517 feet per 15 minutes.

According to series No. 2, Table I., the heat due to 0·983 of current magneto-electricity is $\left(\frac{983}{902}\right)^2 \times 1^\circ{\cdot}56 = 1^\circ{\cdot}85$. But as the resistance of the coil of the revolving electro-magnet was to that of the whole circuit as 1 : 1·13, the heat evolved by the whole conducting circuit was $1^\circ{\cdot}85 \times 1{\cdot}13 = 2^\circ{\cdot}09$. Adding to this $0^\circ{\cdot}33$ on account of the heat evolved by the iron of the revolving electro-magnet, and $0^\circ{\cdot}04$ on ac-

count of the sparks* at the commutator, we have a total of $2^{\circ}{\cdot}46$. Now in order to refer this to the capacity of a lb. of water, I found—

	lbs.		lbs.	
Weight of glass tube	= 1·65 =	capacity for heat of	0·300	of water.
Weight of water	= 0·61 =	... ...	0·610	...
Weight of electro-magnet	= 1·67 =	... ...	0·204	...
Total weight ...	= 3·93 =	... ...	1·114	...

$2^{\circ}{\cdot}46 \times 1{\cdot}114 = 2^{\circ}{\cdot}74$; and this has been obtained by the power which can raise 4 lbs. 12 oz. to the perpendicular height of 517 feet.

1° of heat per lb. of water is therefore equivalent to a mechanical force capable of raising a weight of 896 lbs. to the perpendicular height of one foot.

Two other experiments, conducted precisely in the same manner, gave a degree of heat to mechanical forces represented respectively by 1001 lbs. and 1040 lbs.

I now made an experiment similar to those of series No. 10. Eight cells in a series of four double pairs were connected with the large electro-magnet, and two in series with the small revolving electro-magnet. The velocity of revolution was at the rate of 640 per minute, contrary to the direction of the attractive forces, causing the needle to be deflected to 37° 20′, which indicates a current of 1·955.

A weight of 6 lbs. 4 oz. placed in each scale was just able to maintain the above velocity when the circuits were complete; but when they were broken, and friction alone opposed the motion, a weight of 2 lbs. 8 oz. only was required, which is less than the former by 3 lbs. 12 oz. The fall of the weights was in this instance 551 feet per 15 minutes.

According to series 10, Table II., the heat due to the current observed in the present instance is $\left(\frac{1{\cdot}955}{1{\cdot}845}\right)^2 \times 5^{\circ}{\cdot}88 = 6^{\circ}{\cdot}6$. But I had found by calculations, based as usual upon the laws of Ohm, that, in the present experiment, the resistance of the coil of the revolving electro-magnet was to that of the whole circuit, including the two cells, as 1 : 1·303. Therefore the heat evolved by the whole circuit, including $0^{\circ}{\cdot}18$ on account of the iron of the revolving electro-magnet, and $0^{\circ}{\cdot}12$ on account of sparks at the commutator, was $8^{\circ}{\cdot}9$, or $9^{\circ}{\cdot}92$ per capacity of a lb. of water.

Now when the revolving electro-magnet was stationary, the two cells could pass through it an uniform current of 1·483.

* The heat evolved by sparks in the above and subsequent instances had been determined by previous experiments.

The heat evolved from the whole circuit by such a current is $\left(\frac{1\cdot483}{2\cdot145}\right)^2 \times 5^\circ\cdot88 \times 1\cdot303 \times 1\cdot114 = 4^\circ\cdot08$ per lb. of water per 15 minutes, according to data previously given. Hence the quantity of heat due to the chemical reactions in the experiment is $\frac{1\cdot955}{1\cdot483} \times 4^\circ\cdot08 = 5^\circ\cdot38$, instead of $9^\circ\cdot92$, the quantity actually evolved.

Hence 4° 54′ were evolved in the experiment over and above the heat due to the chemical changes taking place in the battery, by the agency of a mechanical power capable of raising 7 lbs. 8 oz. to the height of 551 feet. In other words, one degree is equivalent to 910 lbs. raised to the height of one foot.

An experiment was now made, using the same apparatus as an electro-magnetic engine. The power of the magnetic attractions and repulsions alone, without the assistance of any weights, was able to maintain a velocity of 320 revolutions per minute. But when the circuits were broken, a weight of 1 lb. 2 oz. had to be placed in each scale in order to obtain the same velocity. The deflection of the needle was in this instance $17^\circ\ 15' = 0\cdot63$ of current electricity. The perpendicular descent of the weights was 275 feet per 15 minutes.

Now, calculating in a similar manner to that adopted in the last experiment, we have, from series 9, Table II., and other data previously given, $\left(\frac{630}{543}\right)^2 \times 0^\circ\cdot50 \times 1\cdot303 = 0^\circ\cdot877$, which, on applying a correction of $0^\circ\cdot012$ on account of sparks at the commutator, and $0^\circ\cdot18$ on account of the iron of the revolving electro-magnet, and then reducing to the capacity of a pound of water, gives $1^\circ\cdot191$ as the quantity of heat evolved by the whole circuit in 15 minutes.

The quantity of current which the two cells could pass through the revolving electro-magnet when the latter was stationary, was in this instance 1·538; and $\left(\frac{1\cdot538}{2\cdot145}\right)^2 \times 5^\circ\cdot88 \times 1\cdot303 \times 1\cdot114 = 4^\circ\cdot38$. Hence, as before, the quantity of heat due to the chemical reactions during the experiment is $\frac{0\cdot63}{1\cdot538} \times 4^\circ\cdot38 = 1^\circ\cdot794$, which is $0^\circ\cdot603$ more than was obtained during the revolution of the electro-magnet.

Hence $0^\circ\cdot603$ has been converted into a mechanical power equal to raise 2 lbs. 4 oz. to the height of 275 feet. In other words, one degree per lb. of water may be converted into the mechanical power which can raise 1026 lbs. to the height of one foot.

Another experiment, conducted in precisely the same manner as the above, gave, per degree of heat, a mechanical power capable of raising 587 lbs. to the height of one foot.

As the preceding experiments are somewhat complicated, and therefore subject to the accumulation of small errors of observation, I thought it would be desirable to execute some of a more simple character. For this purpose I determined upon an arrangement in which the whole of the heat would be evolved in the revolving tube.

The iron cylinder used in previous experiments was placed in an electrotype apparatus constructed in such a manner as to render every part of it equally exposed to the voltaic action. In four days 11 oz. of copper were deposited in a hard compact stratum. The ends of the cylinder were then filed until the iron just appeared. Thus I had a cylinder of iron immediately surrounded by a hollow cylinder of pure copper nearly one-eighth of an inch thick. This was placed in the centre of a new revolving tube fitted up in precisely the same manner as the former one, which had been accidentally broken, and surrounded with 11½ oz. of water. I give the following series of experiments in which the above was rotated between the poles of the large electro-magnet excited by ten cells arranged in a series of five double pairs, a galvanometer being included in the circuit to indicate the electric force to which the electro-magnet was exposed.

No. 16.

		Revolutions of the Bar per minute.	Deflections of Galvanometer of 1 turn.	Mean Temperature of Room.	Mean Difference.	Temperature of Water.		Gain or Loss.
						Before.	After.	
July 5, A.M.	Battery contact broken.	600	° ′ ...	67°·50	0°·15−	67°·37	67°·33	00°·4 loss
July 5, A.M.	Electro-magnet in action.	600	72 35	69·32	0·42−	67·50	70·30	2·80 gain
July 4, P.M.	Battery contact broken.	600	...	68·80	0·16+	69·00	68·93	0·07 loss
July 4, P.M.	Electro-magnet in action.	600	72 25	69·70	0·56+	69·00	71·52	2·52 gain
	Mean, Electro-magnet in action.	600	72 30	...	0·07+	...	...	2·66 gain
	Mean, Battery contact broken.	600	...	...	...	...	...	0·05 loss
	Corrected Result.	600	72° 30′ = 10·93 current.					2·73 gain

I now proceeded to ascertain, by means already described, the mechanical power by which the above effects were produced. First, I ascertained the current passing through the coil of the electro-magnet; then the weights necessary to maintain the velocity of 600 revolutions per minute, both when the magnet was in action and when contact with the battery was broken. I have collected the results of my experiments on this subject in the following table. The first five were obtained with a battery of ten cells in a series of five; the last two with a battery of five pairs in series.

TABLE IV.

	Deflections of the Galvanometer of one turn completing the circuit of the Electro-Magnet.	Weight in each scale, the Electro-Magnet being in action.	Weight in each scale, the Electro-Magnet being not in action.	Difference.
	° ′	lb. oz.	lb. oz.	lb. oz.
	72 30	4 4	2 5	1 15
	72 30	4 4	2 3	2 1
	72 25	4 2	2 0	2 2
	72 15	5 0	2 10	2 6
	72 5	4 0	2 0	2 0
	68 0	3 14	2 8	1 6
	66 10	3 0	2 0	1 0
Mean of the first 5 experiments	$72° 21' = 10·82$ current.			2·1 lbs.
Mean of the last 2 experiments	$67° 5' = 7·91$ current.			1·19 lb.

Referring to series 16, we see that $2°·73$ were obtained when the bar was revolved between the poles of the electro-magnet excited by a current of 10·93. Therefore the quantity of heat due to the mean current in the first five experiments of the above table is $\left(\frac{10·82}{10·93}\right)^2 \times 2°·73 = 2°·675$. To reduce this to the capacity of a pound of water, I had in the present instance the following data:—

	lbs.		lb.	
Weight of glass tube .	= 1·125 =	capacity for heat of	0·205	of water.
Weight of water . .	= 0·687 =	... ...	0·687	...
Weight of metallic bar	= 1·688 =	... ...	0·202	...
Total Weight	= 3·500 =	... ...	1·094	...

$2°·926$, the product of 1·094 and $2°·675$, is therefore the heat generated by a mechanical force capable of raising 4·2 lbs. to the height of 517 feet.

In other words, one degree of heat per lb. of water may be generated by the expenditure of a mechanical power capable of raising 742 lbs. to the height of one foot.

By a similar calculation, I find the result of the last two experiments of the table to be 860 lbs.

The foregoing are all the experiments I have hitherto made on the mechanical value of heat. I admit that there is a considerable difference between some of the results, but not, I think, greater than may be referred with propriety to mere errors of experiment. I intend to repeat the experiments with a more powerful and more delicate apparatus. At present we shall adopt the mean result of the thirteen experiments given in this paper, and state generally that,—

The quantity of heat capable of increasing the temperature of a pound of water by one degree of Fahrenheit's scale is equal to, and may be converted into, a mechanical force capable of raising 838 *lbs. to the perpendicular height of one foot.*

Among the practical conclusions which may be drawn from the convertibility of heat and mechanical power into one another, according to the above absolute numerical relations, I will content myself with selecting two of the more important. The former of these is in reference to the duty of steam-engines; the latter, to the practicability of employing electro-magnetism as an œconomical motive force.

1. In his excellent treatise on the Steam-engine, Mr. Russell has given a statistical table*, containing, among other important matter, the number of pounds of fuel evaporating one cubic foot of water, from the initial temperature of the water, and likewise from the temperature of 212°. From these facts it appears that in the Cornish boilers at Huel Towan, and the United Mines, the combustion of a lb. of Welsh coal gives 183° to a cubic foot of water, or otherwise 11,437° to a lb. of water. But we have shown that one degree is equal to 838 lbs. raised to the height of one foot. Therefore the heat evolved by the combustion of a lb. of coal is equivalent to the mechanical force capable of raising 9,584,206 lbs. to the height of one foot, or to about ten times the duty of the best Cornish engines.

2. From my own experiments, I find that a lb. of zinc consumed in Daniell's battery produces a current evolving about 1320°; in Grove's battery, about 2200° per lb. of water. Therefore *the mechanical forces of the chemical affinities which produce the voltaic currents in these arrangements, are, per lb. of zinc, equal respectively to* 1,106,160 *lbs. and* 1,843,600 *lbs. raised to the height of one foot.* But since it will be practically impossible to convert more than about one half of the heat of

* Enc. Brit., 7th Edition, vol. xx. part 2. p. 685.

the voltaic circuit into useful mechanical power, it is evident that the electro-magnetic engine, worked by the voltaic batteries at present used, will never supersede steam in an œconomical point of view.

Broom Hill, Pendlebury,
near Manchester, July 1843.

P.S.—We shall be obliged, after all, to admit that Count Rumford was right in attributing the heat evolved by boring cannon to friction, and not (in any considerable degree) to any change in the capacity of the metal. I have myself proved experimentally that *heat is evolved by the passage of water through narrow tubes.* My apparatus consisted of a piston perforated by a number of small holes, working in a cylindrical glass jar containing about 7 lbs. of water. I thus obtained one degree of heat per lb. of water from a mechanical force capable of raising about 770 lbs. to the height of one foot,—a result which will be allowed to be very strongly confirmatory of our previous deductions. I shall lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, *indestructible*; and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained.

On conversing a few days ago with my friend Mr. John Davies, he told me that he had himself, a few years ago, attempted to account for that part of animal heat which Crawford's theory had left unexplained, by the friction of the blood in the veins and arteries, but that, finding a similar hypothesis in Haller's 'Physiology*,' he had not pursued the subject further. It is unquestionable that heat is produced by such friction, but it must be understood that the mechanical force expended in the friction is a part of the force of affinity which causes the venous blood to unite with oxygen; so that the whole heat of the system must still be referred to the chemical changes. But if the animal were engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that in proportion to the muscular effort put forth for the purpose, a *diminution* of the heat evolved in the system by a given chemical action would be experienced.

I will observe in conclusion, that the experiments detailed in the present paper do not militate against, though they certainly somewhat modify the views I had previously entertained with respect to the electrical origin of chemical heat. I had before endeavoured to prove that when two atoms combine together, the heat evolved is exactly that which would have been evolved by the electrical current due to the chemical action taking place, and is therefore proportional to the intensity of the

* Haller's Physiology, vol. ii. p. 304.

chemical force causing the atoms to combine. I now venture to state more explicitly, that it is not precisely the attraction of affinity, but rather the mechanical force expended by the atoms in falling towards one another, which determines the intensity of the current, and consequently the quantity of heat evolved; so that we have a simple hypothesis by which we may explain why heat is evolved so freely in the combination of gases, and by which indeed we may account "latent heat" as a mechanical power prepared for action as a watch spring is when wound up. Suppose, for the sake of illustration, that 8 lbs. of oxygen and 1 lb. of hydrogen were presented to one another in the gaseous state, and then exploded, the heat evolved would be about one degree Fahr. in 60,000 lbs. of water, indicating a mechanical force expended in the combination equal to a weight of about 50,000,000 of lbs. raised to the height of one foot. Now if the oxygen and hydrogen could be presented to each other in a liquid state, the heat of combination would be less than before, because the atoms, in combining, would fall through less space. The hypothesis is, I confess, sufficiently crude at present, but I conceive that ultimately we shall be able to represent the whole phænomena of chemistry by exact numerical expressions, so as to be enabled to predict the existence and properties of new compounds.

August, 1843. J. P. J.

LIII. *Experiments on Voltaic Reaction. By* W. R. Grove, *Esq., M.A., F.R.S., Professor of Experimental Philosophy in the London Institution**.

ON the weekly evening meeting of the Royal Institution for March 13, 1840†, I communicated some experiments and observations on certain phænomena which I collated under the general term Voltaic Reaction. I then stated, that in certain (probably in all) cases of the development of a voltaic current a reaction was induced by the voltaic force itself, and that upon the cessation of the initial force the reacting force was apparent in an opposed direction. I showed, moreover, that the diminution or removal of this reaction was one means of increasing the power of the initial current. This reaction in electrolytes (though it is by no means confined to electrolytes) is what has been generally called polarization, and would be one of the resistances to be taken into account in calculating the resulting power of a voltaic current upon Ohm's theory.

It recently occurred to me, that as one method of increasing the power of the initial current was to diminish (or, as it were,

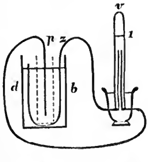
* Communicated by the Author.

† A report is published in the Phil. Mag., S. 3., vol. xvi. p. 338.

absorb) this reaction, so another method of effecting the same object would be, to add the reacting to the initial force, which, from the separable character of the former, did not appear impracticable. After sundry devices the following experiments realized my views on this subject.

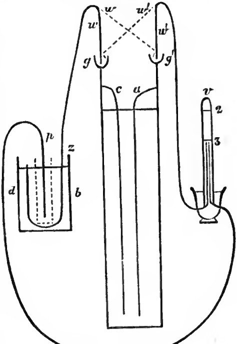
Experiment 1. fig. 1.—*d b* is a single cell of the nitric acid battery, exposing six square inches of each metal; *v* is an ordinary voltameter, each electrode exposing half a square inch, charged with dilute sulphuric acid; decomposition was allowed to proceed with this arrangement for six hours; the battery, for greater assurance of constancy, being in this and the two following experiments recharged every two hours; the level of the liquid in the voltameter was carefully marked on the tube.

Fig. 1.



Experiment 2. fig. 2. is the *same* nitric acid battery, *d b* the *same* voltameter *v*, but with an interposed pair of large platinum plates, *a c*, exposing each to each forty-two square inches of surface, and immersed in dilute sulphuric acid; this arrangement was also set to work for six hours. A slight evolution of gas had taken place in the voltameter in this experiment, and the water-level was also marked.

Fig. 2.



Experiment 3.—The same apparatus as fig. 2; but my assistant was directed to change at

a certain interval the wires dipping into the mercury cups $g\,g'$, so as to reverse the plates $a\ c$, with regard to the direction of the current, making what was the anode the cathode, and *vice versâ*, as shown by the dotted lines; and at the expiration of a similar interval to restore them to their original positions, and to continue thus alternating the position of these plates with reference to the current during six hours.

The interval was to be dependent upon the following observation:—When first the circuit was completed, a marked evolution of gas was perceptible in the voltameter, this gradually subsided, and when it had become nearly imperceptible the change was to be made, when a fresh burst of gas took place, as this again subsided the wires were to be again changed, and so on. At the expiration of six hours the water-level was marked, as in the previous experiments.

The following is the quantity of gas evolved in the voltameter, deduced from a mean of several experiments:—

	cubic inch.
Experiment 1.	= 0·15
Experiment 2.	= 0·10
Experiment 3.	= 0·23.

In neither of the last two experiments was a bubble of gas perceptible on the large plates $a\ c$.

It appears from these experiments, that the nitric acid battery will decompose water across two pairs of interposed inoxidable electrodes, provided one be of considerable size with reference to the other parts of the circuit, so as to lessen resistance.

Whether this diminished resistance be occasioned by the mere increase of the sectional area of the electrolyte; whether by the increased facility for solution of the oxygen and hydrogen; whether the oxygen and hydrogen be not eliminated, but merely thrown into a state of polar tension, or made to adhere in a liquid or gaseous form to the plates; or whether any of these effects take place conjointly, I will not stop to inquire, but proceed to the more remarkable fact, viz. that the quantity of gas evolved in Experiment 3. is greater for a given time, not only than that evolved in Experiment 2, but even than that evolved in Experiment 1; thus we get the seeming paradox, that a battery performs more work with an interposed resistance than without it.

While the battery is decomposing water in the voltameter v, it is polarizing the plates $a\ c$, or accumulating by its own force an antagonist force; when the wires are changed this reacting force is united in direction with the initial force, in fact two voltaic pairs are constituted. The reaction being ex-

hausted, a new polarization commences, to be added in its turn to the initial current; and the reason why we get the increased work at the voltameter is, that *while* the polarization is proceeding at *a c* water is decomposed in the voltameter, and although this may be somewhat less than the battery would produce without the interposed plates *a c*, still this deficiency is more than made up by the action of the double pair at each alternation of the wires. If the view I have taken be correct, as reaction can never be greater than the action which occasions it, we should never get, in Experiment 3, beyond the quantity of gas given by two pairs of the battery *d b*, but we may indefinitely approach that maximum.

A commutator might be easily arranged instead of the hand for effecting the alternation at the proper periods, which, by a little contrivance, may be made to work by the battery itself, but I prefer stating the experiment in its most simple form and free from mechanical complication.

Although the experiment as here described is merely in illustration of a principle, it appears to me to promise results of some practical value; the œconomy of this method of applying force is evident, we get all but a double product with a single consumption; the principle in all probability is not confined to the voltaic force, but may perhaps be applied to mechanics.

LIV. *Occasional Notes on Indications of the Barometer and Thermometer during Stormy Weather at Belfast, from November* 1833 *to January* 1843. *By the Rev.* WILLIAM BRUCE*.

1833. Nov. 28 and 29. ONE of the severest storms that had been recollected. Wind began at about south-east, changed during night to north-west or west-north-west with tremendous rain. Barometer rose from 5 o'clock p.m. of November 28, full one inch and a half in thirty-six hours, having previously fallen with great rapidity, but the quantity not noted. By the Library Barometer there was a rise of 1·37 in forty-five hours, viz. from 2 o'clock p.m. on November 28 to 11 o'clock a.m. on November 30, but no account could there be taken of the additional fall from 2 o'clock to 5 o'clock, when the rise began.

1834. Nov. 5.—Barometer continued falling during the whole day till 11 o'clock p.m. On the morning of the 6th it had risen about four-tenths, and continued to rise till 11 o'clock p.m. (a beautiful day); on the morning of the 7th it had fallen nearly

* Communicated by the Author.

half an inch, with a tremendous storm of wind and rain, and continued falling till night; on the morning of the 8th it had risen three-tenths, and continued rising until night, when it was up half an inch higher than the preceding night, and so continued rising for several days.

1835. Jan. 17.— Thermometer exposed with northern aspect, stood at 20° at 10 o'clock p.m.

Jan. 20.—Thermometer exposed with northern aspect, stood at 24° at 9 o'clock p.m.

Jan. 23.—Thermometer exposed with northern aspect, stood at 48° at 10 o'clock p.m.

Jan. 25.—Thermometer exposed with northern aspect, stood at 51° at 9 o'clock p.m.

Feb. 1.—Thermometer exposed with northern aspect, stood at 56° at 4 o'clock p.m.

1836. Dec.—Between $10\frac{1}{2}$ o'clock p.m. of the 13th, and $10\frac{1}{2}$ o'clock p.m. of the 14th, the barometer rose from 29° to 30°, one inch in twenty-four hours.

*1839. Jan. 6.—Storm began about $10\frac{3}{4}$ o'clock; barometer falling rapidly for some time before. It fell to 28·15 before 1 o'clock of the morning of the 7th, between which hour and $7\frac{1}{2}$ o'clock a.m. it had risen to 28·62. Thermometer had stood at 31° at 9 o'clock a.m. of the 6th, and rose to 50° at 10 o'clock p.m. of the same day. I calculated that it fell one inch and then rose half an inch in twelve hours' time. At $9\frac{1}{4}$ o'clock a.m. of the 7th, barometer stood at 28·75; at $10\frac{1}{2}$ o'clock a.m. of the 7th, barometer stood at 28·81. Began to fall again at $11\frac{1}{4}$ o'clock a.m; at $1\frac{1}{2}$ o'clock at 28·90; at 4 o'clock p.m. stood at 29·05.

On Tuesday morning, the 8th inst., at $7\frac{1}{4}$ a.m., the barometer stood at 29·60, being a *rise* of $1\frac{1}{2}$ inch from Monday morning at 1 o'clock a.m. to Tuesday at 7 o'clock a.m. = thirty hours.

Feb. 1.—At $7\frac{1}{2}$ o'clock a.m., thermometer stood at 21°.

Feb. 8.—At 8 o'clock p.m., thermometer stood at 54°, with a very high wind.

Feb. 13.—At night and morning of 14th, barometer 29·85; thermometer 44°. At 11 o'clock p.m., in five hours after, viz. at 4 o'clock a.m., barometer 29·55; thermometer 51°. At 7 o'clock a.m. of the 14th, barometer 29·60; thermometer 47°. At 9 o'clock a.m., barometer 29·70. On the 15th and 16th a storm of snow and wind.

1840. Jan. 18.—From 11 o'clock p.m. to Jan. 19, at same hour, the range of the barometer was about eight-tenths of an inch, being a fall of five-tenths and a rise of three-tenths, with a gale of wind and severe showers.

Jan. 26.—From 4 o'clock p.m. to 27th, at same hour, a range of fully eight-tenths' rise, with high winds and showers of sleet and snow.

Oct. 18.—Barometer fell more than four-tenths. On the 19th, at 7 o'clock a.m., it had risen more than two-tenths; between this and 11 o'clock a.m., it had fallen again about a quarter of an inch, and between 11 and $12\frac{1}{2}$ o'clock it rose nearly as much. Wind very high from west-south-west to north-west.

Nov. 12.—At night, and morning of the 13th (Thursday and Friday), there was a severe gale of wind with tremendous rain from north-east. South coast of England had the same storm from south-west.

Nov. 15th (Sunday).—At night very heavy rain, and on Monday night (16th), a very severe gale of wind from south-west to west. Rise of barometer seven-tenths.

Nov. 20 and 21 (Friday and Saturday).—On night of 20th and morning of 21st, tremendous rain, with a gale of wind veering from south-west to west. Fall of barometer more than five-tenths in nine hours. The wind had been north to south on Thursday night and Friday morning.

Dec. 6 and 7.—A very severe gale of wind, and some rain from south-south-west to west-south-west or west; and on Tuesday preceding a severe gale also. Thermometer at 55° and 50°.

1841. Jan. 3.—On last night and this morning a very severe gale in tremendous gusts, beginning at south of west, and passing to west and north-west; thermometer in the course of a very few hours sinking from 44° to 32°. Gale continued all day of the 3rd, and also on the 4th, with a rise of the barometer = to about eight-tenths or more in twenty-four hours. Wind on the 4th, north-west and north-north-west. Sky electrical in appearance. On the 8th of January, and several days after, thermometer stood at about 20°.

Jan. 16th.—At night a severe gale of wind, beginning early in the evening at east, or south-east, and going round by south to south-west and west-south-west or west. Barometer fell five-tenths in ten hours.

March 30.—At night and following morning a very severe gale of wind, beginning at westward of south, and changing to west with tremendous rain.

May 1.—Thermometer at 71° in the shade, north aspect, at 3 o'clock p.m.; on the next day, at same hour, 48°; difference 23 degrees.

Oct. 17.—At night and following morning a severe gale, beginning at south or south-south-west and ending at north-west.

Barometer rose between $10\frac{1}{2}$ o'clock at night and $7\frac{1}{2}$ o'clock next morning (nine hours) from 28·95 to 29·75 = eight-tenths; it had been falling pretty rapidly for some time before.

Dec. 5.—Last night a gale of wind with rain from northward of west. Barometer *rose* about 1 inch in twenty-four hours.

Dec. 6.—Another gale with heavy rain. Barometer *fell* half an inch in twenty-four hours.

Dec. 12.—A severe gale early this morning. Barometer fell four-tenths in nine hours, and thermometer rose from 40° to 50° in the course of the night.

Dec. 14.—A gale last night from north-west. Barometer had fallen yesterday morning about three-tenths; between 4 and 5 o'clock it began to rise, and had risen eight-tenths at 8 o'clock this morning.

Dec. 15.—Another gale last night with heavy showers. Wind south-west. Barometer fell more than four-tenths in the night.

Dec. 19.—Thermometer last night at $10\frac{1}{2}$ o'clock stood at 19°, next morning at 8 o'clock at 28°.

1842. Jan. 25.—This night, and the morning of the 26th, a remarkably severe gale of wind with tremendous rain, particularly between 5 o'clock and 7 o'clock. The barometer fell nine-tenths of an inch in nine hours, viz. from 11 o'clock p.m. to 8 o'clock a.m.; at 11 o'clock it stood at 29·7, and at 8 o'clock it was at 28·8, still falling. Wind south, veering to south-west and back. In the evening of the 26th, about 3 o'clock, the barometer began to rise, and for some hours rose at the rate of one-tenth per hour, with the wind north-north-west and a severe gale of wind and rain.

Feb. 27.—About 4 o'clock a.m. a very severe gale of wind with heavy rain. On the preceding day the barometer was rising till 10 o'clock p.m., when I last observed it. At 8 o'clock on the following morning it had fallen five-tenths, viz. in ten hours. Heavy squalls all day on Sunday; worst part of the storm nearly due west, sometimes west-south-west.

Oct. 22.—A severe gale last night; barometer fell half an inch in nine hours.

Dec. 15.—Last evening a heavy gale of wind from south-west or west-south-west; no remarkable fall of the barometer, but the heat unnatural; thermometer being 54°, succeeded by a fall of rain without any wind during the whole of this day and night.

Dec. 22.—Last night and this morning a gale of wind at south-west; barometer fell one quarter of an inch; thermometer at 8 o'clock a.m. 50°.

Dec. 26.—Last night a gale; wind west-south-west to west; fall in barometer about two-tenths, or two and a half; thermometer 48° at night, 45° in the morning.

Dec. 29.—Last night a gale, continuing in the morning; wind west-south-west. The fall in the barometer at the commencement was not more than one-tenth of an inch, and during the night it did not fall at all, standing during the gale at 29·9. Thermometer yesterday morning at 34°, with a hard frost on the ground, which lasted till 11 o'clock a.m. At 10 o'clock p.m. thermometer was 48° (a difference of 14°), and this morning at 50°.

Dec. 30.—Last night, but more particularly from 3 o'clock this morning, very heavy gusts of wind nearly west, without any rise or fall in barometer worth notice; but the temperature at night was 50°, and at half-past 7 o'clock this morning 52°. About half-past 6 o'clock in the morning I thought I saw patches or gleams of electric light along the grass. Gale continued throughout the day, barometer rising very slowly.

Dec. 31.—Same storm continued all last night, with thermometer at 54° at half-past 10 o'clock p.m., and at 56° at 8 o'clock this morning; barometer did not vary more than half a tenth, standing still at 30°. A drizzling rain and damp atmosphere so much illuminated that I could see my watch at any hour, though there were no stars and the moon at the change. I observe in the papers an account of a very extraordinary thunderstorm, doing much damage to houses and cattle at Ballyshannon, on Friday the 30th.

In the evening of the 31st it became calm and the air cooler; weather continued very fine for two or three days.

1843. Jan. 13.—Last night barometer fell seven-tenths of an inch between 11 o'clock p.m. and 8 o'clock this morning (nine hours), when it was lower than I had almost ever seen it, except in Jan. 1839. A very heavy squall of wind and rain during the night, wind veering from south-east to south or south-west. Morning calm, but showers of snow during the day, and from 8 o'clock p.m. the wind began to blow a gale. I have heard from Dublin that the barometer was so low as 27·9, and I see in our own papers notices of an extraordinary state of the tides.

LV. *New Criteria for the Imaginary Roots of Numerical Equations.* By J. R. Young, *Professor of Mathematics in Belfast College**.

IN a former Number of this Journal, and with fuller detail in a more recent publication†, I have discussed some useful formulæ for discovering imaginary roots in an equation from inspecting the coefficients of its terms. The new forms

* Communicated by the Author.

† Researches respecting the Imaginary Roots of Equations.

proposed in the present paper will, I think, prove an acceptable addition to those previously given: they are not only of greater simplicity, but will often succeed in detecting the presence of imaginary roots in cases where Newton's formulæ—the basis of those adverted to above—would prove inefficient. These new forms are deduced as follows:—

Let the equation

$$A_n x^n + A_{n-1} x^{n-1} + A_{n-2} x^{n-2} + A_{n-3} x^{n-3} + \ldots A_1 x + A_0 = 0$$

be multiplied by $x - a$, that is, let a new undetermined real root a be introduced into the equation: whatever imaginary roots are indicated in the new equation, the same of course enter into the original. This new equation is

$$A_n x^{n+1} + (A_{n-1} - a A_n) x^n + (A_{n-2} - a A_{n-1}) x^{n-1}$$
$$+ (A_{n-3} - a A_{n-2}) x^{n-2} + \ldots (A_0 - a A_1) x + a A_0 = 0,$$

in which a is entirely arbitrary, and may therefore be made to satisfy any condition we please. It may for instance be determined so as to render any one of these compound coefficients zero; and if, in conjunction with this determination of a, the original coefficients be so related as to cause the compound coefficients on each side of this zero to be of like signs, we shall at once recognise the presence of imaginary roots in the proposed equation.

Equating then the several coefficients to zero, one after the other, commencing with the second, and determining the adjacent pair of coefficients in each case conformably to this condition, we shall have the following sets of conditions, the existence of any one of which will imply a pair of imaginary roots:—

$$A_n = +, \quad A_{n-1} - a A_n = 0, \quad A_n A_{n-2} - A^2_{n-1} = +,$$
$$A^2_{n-1} - A_n A_{n-2} = +, \quad A_{n-2} - a A_{n-1} = 0, \quad A_{n-1} A_{n-3} - A^2_{n-2} = +,$$
$$A^2_{n-2} - A_{n-1} A_{n-2} = +, \quad A_{n-3} - a A_{n-2} = 0, \quad A_{n-2} A_{n-4} - A^2_{n-3} = +,$$
$$\&c. \qquad \&c. \qquad \&c.$$

And therefore, a being always assumed so as to satisfy one of the middle equations, those on each side will furnish the following series of criteria of imaginary roots, viz.—

$$A_n = +, \qquad A_n A_{n-2} > A^2_{n-1} \ldots [1.]$$
$$A^2_{n-1} > A_n A_{n-2}, \quad A_{n-1} A_{n-3} > A^2_{n-2} \ldots [2.]$$
$$A^2_{n-2} > A_{n-1} A_{n-3}, \quad A_{n-2} A_{n-4} > A^2_{n-3} \ldots [3.]$$
$$\&c. \qquad \&c.$$

Now assuming, according to custom, that A_n is always plus, it is plain, that if any one of the right-hand conditions have place, without regarding those on the left, a pair of imaginary roots will necessarily be indicated; because, although the accompanying left-hand condition should not have place, yet, by ascending to the preceding pair of conditions, and thence to that next in order, and so on, we should evidently at length arrive at a co-existent pair. It is equally evident that no two consecutive pairs can exist simultaneously, since the second condition in any one pair is opposed to the first in the pair next following. Hence, although all the right-hand conditions marked [1.], [2.], [3.], &c. were found to have place, yet only one pair of imaginary roots could be inferred: these conditions, therefore, can be regarded only as so many concurrent indications of the same thing. Whenever this concurrence ceases, by a failure of one of the right-hand conditions referred to, or which is the same thing, by a fulfilment of the left-hand condition next in order, then preparation is made for a new indication, totally distinct from those that have preceded; and if such new indication offer itself, a distinct pair of imaginary roots may be inferred.

Hence the series of conditions [1.], [2.], [3.], &c. furnish us with criteria of imaginary roots somewhat analogous to those of Newton; much simpler, however, in form, but to be employed exactly in the same manner. I shall give two examples of this application:—

1st. $5x^8 - 2x^7 + 3x^6 - 24x^5 - 16x^4 + x^3 - 4x^2 - 2x - 60 = 0.$

Applying the criteria to this equation, we discover the existence of six imaginary roots; the same as by the rule of Newton (Researches, &c., p. 47).

2nd. $9x^5 - 5x^4 + 4x^3 - 3x^2 + 6x + A_0 = 0.$

In this example Newton's rule detects only a single imaginary pair; the criteria above discover *two* pair, so that the equation has but one real root.

It may be added that, as in the rule of Newton, the sign $>$ may be changed into $=$.

Belfast, November 7, 1843. J. R. Young.

LVI. *Notices respecting New Books.*

1. *Philosophical Theories and Philosophical Experience.*
2. *Connection between Physiology and Intellectual Philosophy.*
3. *On Man's power over himself to prevent or control Insanity.* Pickering.

At a time like the present, when the general spread of education has put the keys of knowledge into the hands of many, to

whom half a century back they were utterly inaccessible, it appears a strange anomaly that there should be less of serious and deep thought than in the days when little adventitious aid to study of any kind could be obtained; yet those who have observed most closely, and reflected most deeply, agree in the opinion that such *is* the case, and that the present age, with all its "appliances and means" of knowledge, may be justly termed superficial. One obvious cause of such a state of things, and the only one which it is needful to our present purpose to notice, arises from the immense increase of our population—all struggling for maintenance, or for the good things of this world in some shape—all ashamed not to possess some *slight* acquaintance with the literature of the day, yet all too much engrossed by the immediate pressure of their daily concerns to give more than a passing thought to other matters. Among the many whose cases come under the above description, there must however be a large number who would gladly obtain more solid information on subjects, both of science and general knowledge, than has hitherto been within their grasp; but how can the evening of a day of toil, or a proper use of the needful and holy rest of Sunday, afford leisure for the study of voluminous writings, even were the demand on their pockets within the means of those who would learn if they could? The strong conviction of the necessity of furnishing persons so situated with the mental nourishment they require in a condensed form, has induced a few friends to unite "for the purpose of supplying the deficiency," which, as their prospectus states, "could hardly be supplied in the common course of trade;" and the three little works, whose names stand at the head of this article, have been already published by the Society. We willingly co-operate in a design so well intentioned, and so likely, we think, to be useful to the world, by introducing them to the notice of our readers, to whom, however, it is very probable that the 2nd and 3rd numbers may be already known, being the substance of two lectures delivered, one in 1841, the other in the spring of this year, at the Royal Institution, by the Rev. John Barlow.

In the first treatise, "Philosophical Theorics and Philosophical Experience," the 'Pariah' (as the author chooses to style himself) discourses on those most important of all subjects, the present and future destiny of man; and shows by a train of lucid reasoning that all true religion and all real philosophy *must* accord, since eternal truth is One, and admits of no contradictions; and that the word of God and reason unite in pointing out to man wherein his real good consists. "The questions," says the 'Pariah,' "which have agitated mankind in all ages, and whose solution forms the basis of all systems of religion and philosophy, ... may be resolved into three.

"1. What is the nature of the power exterior to ourselves?

"2. What is the nature of the power within ourselves?

"3. What, with reference to these two, is the nature of the good which man ought to propose to himself as his aim and object?" (*Phil. Theo. and Phil. Ex.* pp. 11, 12.)

In reply to the first query the author proves, that "by a legitimate

course of reasoning we arrive at the certainty of one eternal, self-existent, all-wise, and all-powerful Being, whom our simple ancestors, with a degree of philosophical accuracy which no other nation seems to have reached, named ȝob, i. e. good." (Ibid. p. 19.) He shows that "Christianity goes further," but in strict accordance with those conceptions of the Almighty which the purest philosophy could form; and that "it sets before man an exemplar of human virtue, made perfect by the indwelling of the Deity." (Ibid. p. 41.)

In the solution of the second inquiry the 'Pariah' classifies "the phænomena of man's nature into

"1. The instinctive emotions and appetites.

"2. The faculties.

"3. The will." (Ibid. p. 54.)

The two former, partaking "of the changes which the body undergoes," are assumed to be "bodily;" but "the individual and intelligent will," which can control and restrain the emotions and direct the faculties, and also "that species of memory which forms the consciousness of identity, and which (however ordinary recollections may be impaired by the injury or disease of the brain) never suffers any change from infancy to death" (ibid. p. 37), cannot be *bodily*, and are consequently considered by the 'Pariah' as "spiritual and unchanging functions."

The "practical results" which form the reply to the third query are such as, were they carried out in their full extent (and there is no insurmountable obstacle in the nature of things against it), would render our earth a paradise in comparison with its actual condition.

"I have stated," says the Pariah, "that an essential part of the great Self-existent Cause of all things, is a free and governing will. Man therefore in this bears the image of his Maker; and inasmuch as he partakes in a certain degree of the nature of his Creator, his happiness and his destiny must be of a kind somewhat analogous. The felicity of the Creator (as far as we can judge) must consist in the constant harmony of his nature with his acts. The will to do what is best, and the power to effect it, or, in other words, unbounded knowledge, power and benevolence. Now, though man's finite nature can follow but at humble distance, it *can* follow. He may act in conformity to his nature; he may delight in conferring happiness and in seeking knowledge; and I believe all who have tried the experiment will bear testimony that this course confers even in this life a peace of mind, a joy, even in the midst of the turmoils of the world, which is more akin to heaven than earth." (Ibid. pp. 81, 82.)

The great bearings of the second treatise, "On the Connection between Physiology and Intellectual Philosophy," have been given in the lecture on Insanity, and we cannot do better than avail ourselves of a portion of the lecturer's notice of it. He says,

"Two years ago I had the honour of submitting to you some views with regard to intellectual science, which appeared necessarily to result from the recent discoveries in anatomy. ...... In order to make myself clearly understood, it will be necessary to take a brief view of the structure and functions of the brain and nerves, as explained in my former communication."

The author then gives the usual description of that structure, but

we may turn to the second essay for a summing-up of the whole, sufficient to show the general view of the matter taken by the author. A short account is there given of the arrangement of the nerves, &c., and the different functions they perform.

"Thus we have three distinct systems of nervous mechanism in the living body, each dependent on the other, namely,

"1. The unconscious involuntary nerves of life.

"2. The conductors of external and internal feelings to the brain.

"3. The conveyers of volition *from* the brain to the organs fitted for action; which are respectively termed the *sympathetic*, the *sensitive* and the *motor* nerves." (*Connection between Physiology and Intellectual Philosophy*, p. 11.)

The immediate bearing of the second on the third essay will be shown by the following extracts:—

"We have now traced the human animal through all parts of his structure; we have shown first a system of ganglia and nerves springing from them, by means of which organic life is carried on, and appetites excited for its mainteuance; we have further seen a set of nerves whose terminations are to be found at the base of the brain, which supply the senses by which man communicates with the external world; we have seen another apparatus within the cranium by which these sensations are weighed and examined, and the result of this examination transmitted finally to the motor nerves for execution; altogether forming the most perfect piece of machinery ever constructed; for these nice operations of thought are the work of fibres and fluids contained in them, merely set in motion by the impression made at one part, and thus transmitted through the whole series." (Ibid. pp. 50, 51.)

Are our readers about to take alarm and to exclaim that we are here on the verge of materialism? Let them pause; the facts which the investigations of Müller, Solly, and a host of modern anatomists have *proved*, cannot be gainsayed; but mark the inference the author draws from them.

"Look at the astronomer in his observatory! the night is far advanced, and he is chilled and fatigued, yet he remains with his eye at the telescope; for what? to carry on a series of observations which perhaps in two generations more may give as its result the knowledge of some great law of the material universe; but he will be in his grave long ere he can expect that it will be ascertained. He sits down to his calculations and he forgets his meals, sees nothing, hears nothing, till his problem is solved! No sense prompts to this sacrifice of rest and comfort. But do we call these persons insane? No, we honour them as the excellent of the earth, and wish that when the occasion comes we may have courage so to die.

"I know but of one solution of the difficulty: there must be some element in man which we have not yet taken account of; some untiring, undying energy, which eludes indeed the fingers and the microscope of the anatomist, but which exercises a despotic sway over the animal mechanism." (Ibid. pp. 53, 54.)

We hardly need inform our readers that the intelligent will is the element here alluded to, nor will it require much penetration to divine how this bears on the subject of the third communication; for in thus establishing the existence of an agent palpably superior to

the bodily functions, even the highest of them, the power of thought (inasmuch as it can control even that), it naturally follows

"That a force which is capable of acting as an acceleration, a retarding or a disturbance of the vital functions, must have no small influence over so delicate an organ as the brain, and accordingly we find paralysis, inflammation, or brain fever, and a variety of other diseases of this kind, produced in many instances by causes purely mental.......Now a force which can produce disease must have some power also in removing or preventing it; and my business tonight will be to endeavour at least to mark out how far this force can be made available to so desirable an object." (*Lecture on Insanity*, p. 9.)

From this it will be clear what is the line of argument taken up with reference to the awful subject of insanity; and it is fully shown that many cases of mental delusion may consist with moral responsibility, and that "nothing but an extent of disease which destroys at once all possibility of reasoning, by annihilating or entirely changing the structure of the organ, can make a man necessarily mad." (Ibid. p. 12.) That there are such states in which the material organs of thought are so affected as to reduce the unhappy person to a mere "helpless machine," is unfortunately but too sure; yet "such extensive structural disease is hardly compatible with life, and is of very rare occurrence." The facts detailed in this latter lecture, and the opinions of Dr. Conolly and others whose attention has been long directed to the subject of insanity, are indeed well worthy the most serious consideration, and more than ever *now*, since this greatest of all human afflictions is said to "have nearly tripled within the last twenty years!" and "that of the cases less than three hundred in one thousand are the result of disease, or of unavoidable circumstances; thus leaving above seven hundred resulting from bodily excess or mental misgovernment." (Ibid. p. 49.)

We have been anxious to show the connection which exists in these "small books," because the three together form an abundant proof of two propositions most essential to the welfare of the human race; the first, that it is the nature of *truth* not to be *solitary*. The discoveries of modern anatomists have placed us in possession of many of the laws which regulate the mechanism of the human frame, even to the workings of those delicate fibres which are found to be the mechanical instruments of thought. What is the result? Are we hereby reduced to acknowledge that we are mere machines? No; we are enabled thereby to classify the phænomena of our nature; for the anatomist and the chemist alike acknowledge that, while tracing the laws of matter, they find a disturbing force which has its source in other causes; which cannot be referred to any conceivable action of those laws,—nay, which is frequently at variance with them; and we thus arrive at that wonderful agency of the will already noticed. Again, what are the observations of those whose attention has been turned to the morbid action of that established mechanism?—that where sufficient *motive* can be adduced, that indomitable *will* is able even here to step in, and at least in some degree control or counteract the diseased action.

The second proposition then which we must arrive at, if the above reasoning be correct, is, that if this mighty will exist in every human being, its *early regulation* is a vital consideration. Could the conviction be once firmly rooted in the mind, that on the training of that will to assimilate itself as much as possible to His from whom it is derived, depends the temporal and eternal welfare of the individual (and who shall say of how many other beings?), would it not be the first care of every one how to govern his own, and to assist in guiding that of all dependent on him?

The compilers of the three essays we have noticed are evidently too well read themselves to hope that they have found for others a *royal road* to science of any kind; but those who have no leisure to pursue even one science in its details, may nevertheless be clearly shown in a short compass how much there is that may be known, and their eyes will thus be opened to a conception of the majesty and beauty of the works of God of which they had little previous idea. We hail the appearance then of these works as the commencement of an enterprize to which all who love their fellow-creatures must wish well—that of inducing men to *think*, and of affording them the means of doing so to good purpose.

> "Ages pass away,
> Thrones fall, and nations disappear, and worlds
> Grow old and go to wreck; the soul alone
> Endures; and what she chooseth for herself,
> The arbiter of her own destiny,
> That only shall be permanent." (*Southey's Roderick.*)

LVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 311.]

June 1, 1842. A PAPER was first read entitled, "Notice of some Experiments on the Electric Currents in Pennance Mine, near Falmouth." By Robert Were Fox, Esq. Communicated by the President.

The Pennance mine is situated in killas, but to its N.W. is a granite range. Two veins have been worked; the more northerly, which is about five feet wide and has a slight northerly dip, to the depth of sixteen fathoms; and the other, which is about two feet thick and dips apparently to the south, to the depth of eight fathoms. Both veins nearly coincide with the magnetic meridian in their horizontal bearing. They abound with arsenical and iron pyrites interspersed with oxide of tin and sulphurets of copper and lead, arranged in many parts in nearly vertical layers parallel to the sides of the veins.

The author was assisted in his experiments by Mr. J. Fox. The apparatus employed was adapted only for experiments on not very feeble electric currents, and consisted of copper wires from $\frac{1}{14}$th to $\frac{1}{20}$th of an inch in diameter, and plates of different metals, with other contrivances for varying the methods of producing contact with the ore-points selected in the veins. The galvanometer had only one needle $2\frac{1}{2}$ inches long, $\frac{1}{3}$th of an inch wide, and $\frac{1}{20}$th thick, having

an agate cup and moving on a steel point. A fine brass wire was coiled forty-eight times round the box which contained the needle.

The ore-points connected with the two extremities of the apparatus were, in some instances, only six or eight fathoms apart, but in others thirty, forty, and even 100. The small portion of the south vein which could be tried produced a deflection in the needle of about 20° from the point of rest, after the circuit had been repeatedly made and broken; the currents passing from east to west through the apparatus. In the north vein the deflections amounted to 45°, 60°, and 80° in different levels, the direction of the currents being the same as in the south vein; and in the eastern portion of the six-fathom-level the needle traversed completely round and continued to revolve a short time after the circuit was broken.

Sulphuret of lead being much more electro-positive than arsenical copper or iron pyrites, contact was made with those ores, generally dry, without affecting the currents, when the ore-points thus varied were near together, and there was no defect in the contact with them. These results were not apparently modified by the method of making the contact, or by the metal employed to effect it, provided an adequate degree of pressure was employed. For instance, a *point* of a copper wire pressed against a given ore-point was mostly as effectual as a *plate* of that metal similarly treated; and when zinc and platinum were successively substituted for copper no change was produced.

It is, therefore, evident, Mr. Fox remarks, that these electric currents were independent of extraneous causes, and were derived from the veins only.

Towards the eastern part of the north mine arsenical pyrites abounded immediutely under the surface. On one extremity of the apparatus being connected with it and the other with an ore-point to the westward in the six-fathom-level, twenty-four fathoms of wire being employed for this purpose, the current, which was from E. to W., deflected the needle fifty to sixty degrees. Again, contact was made with the ore-point in the six-fathom-level by means of a small plate of copper attached to one of the wires and wedged against the ore by a wooden pole, the other copper wire being firmly pressed against the arsenical pyrites at the surface by means of a brass screw passed through a block of wood, which was retained in its place by a pole wedged against it. This arrangement admitted of the screw being loosened in the block, and the metal in contact with the ore-point being changed without inconvenience. Zinc was used after the copper for making the contact with the ore-point, but without producing any modification of the current, which continued to deflect the needle from fifty to sixty degrees; notwithstanding that any action between the copper in the six-fathom-level and the zinc at the surface, if it had existed, would have been in an opposite direction, and have tended more or less to counteract the influence of the actual current. Its energy, however, was sufficient to render a short bar of iron of a horse-shoe form, with several coils of copper wire around it, feebly magnetic, and affect a needle about two inches long, moving on a

pivot in a close box. Each pole of the needle was about three inches from the extremity of the bar, and was deflected about 2° from its point of rest, on the current being made through the coils of wire; and where the direction of the current was reversed, a similar deflection of the needle to the opposite side was produced. The effect, Mr. Fox observes, would have been greater had the experiment been made entirely in the six-fathom-level, where the electric action was stronger, or if the needle had been suspended and not mounted on a pivot.

Having removed the electro-magnet, and other things remaining the same, a glass tube in the form of a V, having moistened clay at the bottom, was placed in the circuit with water in one branch and a solution of sulphate of copper in the other. Small cylinders of copper pyrites, taken from the same piece of ore, were employed to connect these liquids respectively with the opposite wires, the ore at the *positive* end of the wire having been partly dipped in *water*, and that at the *negative* end in the solution of sulphate of copper. The wires were kept at some distance above the level of the liquids, and as a further precaution, the portions not in contact with the ore were coated with sealing-wax. The liquids in both branches were at the same level, and corks were inserted to retain the pyrites in the same positions. This apparatus remained undisturbed for three days, when the column of the solution of sulphate of copper was found to have increased in height at the expense of the water in the other branch, the difference being about one-tenth of an inch. On the copper pyrites in the solution of sulphate of copper being examined, it was found to be partly coated with metallic copper.

Both these effects were, therefore, produced, Mr. Fox observes, solely by means known to exist in the earth, and the experiments, he adds, seem, therefore, to have a direct and unequivocal bearing, not only on the decomposition of metallic salts under the surface, but on the causes which affect the different levels of subterranean springs, and the purification of water from bodies which it may hold in solution.

A memoir "On the Elevation and Denudation of the District of the Lakes of Cumberland and Westmoreland." By William Hopkins, Esq., F.G.S., was then laid before the Society, an abstract of which has already appeared in the Phil. Mag. S. 3. vol. xxi. p. 468.

June 15, 1842.—A paper was first read "On the packing of Ice in the river St. Lawrence; on a Landslip in the modern deposits of its valley; and on the existence of Marine Shells in those deposits as well as upon the mountain of Montreal." By W. E. Logan, Esq., F.G.S.

1. The paper commences with a general description of the river St. Lawrence, from the junction of the Ottawa in Lake St. Louis, above Montreal, to Lake St. Peter, fifty miles below it, with a more particular account of the rapids of Lachine and the Sault Normand, produced by ledges or floors of trap rock. The author then proceeds to give an account of the packing of the ice near Montreal.

The frosts commence about the end of November, and a margin of ice of some strength soon forms along the shores; and wherever the

water is still, it is immediately cased over. The first barrier completed across the river below Montreal is usually formed about Christmas, at the entrance of Lake St. Peter's, where the St. Lawrence is divided into a multitude of channels by low alluvial islands. This barrier is rapidly increased by extensive fields of drift-ice, enormous quantities of which are heaped upon, or forced under, the stationary mass. The space left for the water to flow being thus greatly diminished, a perceptible rise in the river takes place, and by the time that the ice becomes stationary at the foot of St. Mary's current, opposite Montreal, the waters in the harbour have usually risen several feet, and as the packing rapidly proceeds, they soon attain the height of twenty, and sometimes twenty-six feet, above the summer level. It is at this period that the grandest glacial phænomena are presented. In consequence of the packing and piling of the ice, as well as the accumulation of the moistened snow of the season, and the freezing of the whole into a solid body, sometimes more than twenty feet thick, the water suddenly rises, and lifting a wide expanse of the entire covering of the St. Lawrence, urges it forward with terrific violence, piling up the rended masses on the banks of the narrower parts of the river to the height of forty or fifty feet. In front of Montreal is a newly built *revétement*, the top of which is twenty-three feet above the summer level of the river; but the ice broken by it, accumulates on the surmounting terrace, and before the wall was erected the adjacent buildings were endangered, the ice sometimes breaking in at the windows of the second floor, even 200 feet from the margin of the river. In one instance, a warehouse of considerable strength and magnitude, having been erected without due protection, the great moving sheet of river-ice pushed it over as if it had been a house of cards; and in another case, where a similarly situated and equally extensive warehouse, four or five stories high, had been provided with a range of oaken piles placed at an angle of less than 45°, the drift ice rose up the inclined plane, and after meeting the walls of the building, fell back, and formed, in a few minutes, an enormous but protecting rampart. In some years the ice accumulated nearly as high as the roof of the warehouse.

Several of these grand glacial movements take place, sometimes at intervals of many days, but occasionally of only a few hours, the permanent setting being indicated by a longitudinal opening of considerable extent in some part of St. Mary's current. This opening, which is never afterwards frozen over, even when the temperature is 30° below zero of Fahrenheit, is due to the water having formed a free subglacial as well as superficial passage, in consequence of its own action and the cessation in the supply of drifting ice. From this period the waters gradually subside, but seldom or never to their summer level; and when they have attained their minimum, the trough of the St. Lawrence exhibits, Mr. Logan states, a glacial landscape of undulating hills and valleys.

On the banks of the river, near Montreal, is an immense accumulation of boulders, chiefly of igneous rocks, the most abundant consisting of syenite; and multitudes of them are stated to be " tons in

weight." As they appear also above the surface in the shallow parts of the river, Mr. Logan is of opinion that the bed of it likewise teems with them. Their position has been frequently observed to be changed, both in the St. Lawrence and on its banks, at the breaking up of the ice in the spring. Mr. Logan examined, in the autumn of 1841, the boulders between Montreal and Lachine, a distance of nine miles, and again in the spring of the present year (1842), when he missed some which had particularly attracted his attention; but he adds, he may, from not having mapped their position, have inadvertently passed them over. The author then offers some remarks on the power of ice in moving or transporting boulders along the river, and furrowing the surface of the fixed rocks, as well as along the shores; but he is of opinion that the distance is limited to which they may in the latter position be conveyed annually.

It is not only on the immediate banks of the St. Lawrence that boulders abound, as they are spread more or less over the whole island of Montreal, and the plains on the opposite side of the river. Mr. Logan states that he had not examined their position with sufficient accuracy to offer an opinion respecting the causes of their distribution, but they appeared to him to be more abundant in the upper than the lower part of the island, and they are stated to cease altogether not many miles below it; but their size is not less at the limit of their range than elsewhere.

2. *Landslip.*—The country for a considerable distance on both sides of the St. Lawrence, between Montreal and Quebec, is very level, and is generally covered to some depth by a highly levigated deposit composed of clay, sand, and calcareous matter resting on black shale and black and grey limestone belonging to the Silurian system. This flat region or trough is bounded on the north-west by granitic and syenitic hills about 500 feet in altitude; and on the south-east by an undulating picturesque tract, composed of a hard quartzose conglomerate, which crops out from beneath the limestone, and is succeeded by pyritiferous clay-slate. The cleavage of this formation is stated to be from N.E. to S.W., or parallel to the general strike of the beds.

Between Montreal and Lake St. Peter, the banks of the St. Lawrence have generally a height of twenty or thirty feet above the level of the water, but the plains near the margin of the river are occasionally so low as to permit the formation of marshes, and on the southern side the general surface does not apparently attain the same altitude as on the north-western. On this side, at a distance varying from one to six miles from the St. Lawrence, there is a sudden rise of 100 feet, forming the boundary of a terrace which extends to the granitic hills, where a second rise takes place of 200 or 300 feet. The terrace, composed of soft materials, has a very even surface over a great area, being only modified by the protrusion, at a few places, of the Silurian limestone. It is however intersected by the rivers which flow from the granitic range, and which, dashing down from the hills, cut at once into the terrace, nearly to the level of the St. Lawrence. The banks of these tributaries are liable to landslips, and an

extensive one which occurred on the Maskinongé in 1840 is described in this part of Mr. Logan's paper.

The waters of that river, after passing through a series of lakes, are precipitated from the granitic region in a beautiful cascade, and then flow along a deep valley in the terrace, the only interruption to their course being a collection of boulders combined with a mill-dam, producing a fall of about fifteen feet. The valley has a uniform breadth, the distance between the summit of the banks being about 200 yards, and the height of the banks is 120 feet.

The point at which the landslip occurred is nine miles from the granitic hills, and where the river, ten to twenty yards wide, changes its direction from south to west, for 700 yards. The movement commenced about eight o'clock on the morning of the 4th of April 1840, and when the winter snow was still on the ground. The mass of marly clay, first detached, was about 200 yards in breadth and 700 in length; and it was followed, at intervals of a few minutes, by four others. The whole of the area thus affected amounted to about eighty-four acres, and the total length was 1300 yards; but the breadth varied, the narrowest part being nearest to the river, and the widest, equalling 600 yards, a considerable way from it. The moving mass first crossed the stream, and then splitting against the opposite bank, where it averaged a thickness of seventy-five feet, one-half turned up the valley for about three-quarters of a mile, and the other half down it for an equal distance, forming a dam half a league in extent. The whole operation was completed in about three hours. For some time after the movement began the surface of the great masses remained unbroken, and the sugar maple-trees, with which they were covered, preserved, for the greater part, an erect position. Two farm-steads were also carried away, and though the people escaped, the cattle, and other live stock perished with the falling buildings. The masses which moved along the valley had a height of about sixty feet, and their surface was slightly raised, but the front of each terminated in a blunt point which projected in the middle and in the lower part. As these great double-acting ploughshares advanced, they turned up, Mr. Logan says, the soft mud from the bed of the river, casting it on the banks, and producing so intolerable a stench that no one could approach within 100 yards. This odour, he conceives, arose from the sulphuretted hydrogen produced by the decay of vegetable matter.

No sooner was this dam formed than the waters of the Maskinongé began to rise, and the houses, with every other thing composed of wood throughout the whole of the nine miles to the granitic hills, were set afloat. It was two days, however, before the lake thus formed overtopped the barrier; but by October it had transported into Lake St. Peter so great a portion of the *débâcle* that the river was not more than ten feet above its ordinary level.

When Mr. Logan examined, in the subsequent autumn, the spot where the slip took place, the bottom of the widest part of the chasm was thirty feet below the level of the surrounding country; and about 400 yards from the river, where the disturbed district narrowed, there

was a sudden additional descent of fifteen feet. From this point the ground sloped gently to the water's edge. The only vestiges of the original surface then visible were a few patches of grass, and occasionally twenty or thirty yards of wooden fence, the superficies being composed of parallel mounds three or four feet high, and which over the central portion of the area ranged at right angles to the axis of the slip, but along the sides conformably with the bounding outline.

A circumstance connected with the form of the disturbed district Mr. Logan considers worthy of attention. Around the whole of the area, except the most northern extremity, there was, previous to the slip, a depression of the surface, due on the eastern side to the slope of the right bank of the river, and a tract of low land; and on the western side to a dingle traversed by a brook. After the slip, a ridge, not many feet wide, remained between these lower surfaces and the chasm, forming a marginal rim which was broken through at only one point, where it was intersected by a dingle which united with the one on the western side.

The cause of the slip, Mr. Logan is of opinion, was pressure on an inclined plain, assisted by water; and though he was not able to determine the nature of the subsoil, he is of opinion, from a survey of the surrounding country, that it consists of the Silurian limestone, the dip of which, where visible, is in the direction of the slip.

If boulders were at the bottom of a mass moved in the manner of the Maskinongé slip, it is easy to see, Mr. Logan observes, that parallel grooves and a polish on the surface of rocks may not, in all cases, be due to the agency of ice.

3. *Marine Shells on Montreal Mountain.*—After alluding to Mr. Lyell's account of the fossil shells collected by Capt. Bayfield * in the neighbourhood of Quebec, Mr. Logan proceeds to describe briefly the circumstances under which four of the same species of mollusks were found near Montreal. The spot from which they were principally procured is stated to benr very much the character of a raised beach, and was determined barometrically to be 430 feet above the Montreal harbour, or about 460 feet above the Atlantic, the greatest height at which Captain Bayfield's specimens were found, being 300 feet above the level of the Gulf. The above altitude of the Montreal deposit is further stated to be 240 feet above the level of Lake Ontario and 75 feet above the Falls of Niagara, but to fall short of Lake Erie by about 105 feet. The four ascertained species obtained by Mr. Logan are the following:—

1. *Saxicava rugosa*, very abundant to the north of the road to the Côte de Nieges, in a bed of coarse snnd inclined conformably with the side of the hill, and which hns, above it, a layer of pebbles and small boulders. The altitude of this position is 430 feet. The shell occurs also, but not abundantly, above the village of St. Henry, on the road to Lachine, on the top of nn elevated terrace along the bank of the St. Lawrence, and 120 feet above the river; it has been also obtained on the same terrace at Logan's Farm.

* See Phil. Mag. S. 3. vol. xv. p. 399, and Geol. Trans., 2nd Series, vol. vi. p. 135.

2. *Tellina Groenlandica*, which is found abundantly at St. Henry, and to a less extent on Logan's Farm and on "the Mountain."

3. *Tellina calcaria.*—One valve of this shell was picked up on "the Mountain."

4. *Mya truncata.*—Several hinges of this species were obtained at St. Henry.

5. *Mytilus.*—A broken valve was found on "the Mountain."

The first four fossils, Mr. Logan says, he has been informed, were found by Mr. Murchison and M. de Verneuil at Ust Vaga, 250 miles from the White Sea, and 130 feet above its level. He also alludes to Mr. Lyell's comparison of the Quebec shells with species which occur at Uddevalla.

A communication was afterwards made by Dr. Grant, F.G.S., "On the Structure and History of the Mastodontoid Animals of North America."

The chief object of this communication was to point out the structural differences and zoological distinctions of the Mastodons and Tetracaulodons of North America; and the inquiries were instituted in consequence of the favourable opportunity afforded by the temporary exhibition, in this metropolis, of Mr. Koch's large collection of organic remains from the State of Missouri, consisting principally of the relics of these two genera.

After pointing out the important applications of the study of these remains, and the geological relations of Mastodontoid animals, and the discordant opinions of zoologists as to their specific distinctions, Dr. Grant entered into extended details regarding the general structure and the peculiarities of the skeleton in the three principal Mastodontoid genera, Mastodon, Tetracaulodon, and Deinotherium, which are compared with those of the elephant and other allied genera. The fifth section of the memoir is occupied with the description of the development, forms, structure and changes of the dental system of Mastodontoid animals; and each tooth and tusk of the three principal genera are described and compared, and the principal modifications they exhibit according to difference of age, sex, and species. After pointing out the necessity of including the entire series of successive teeth, in the dental formulæ of genera, where the teeth are constantly displacing and succeeding each other through the whole of life, the author announces the dental formulæ of the four Proboscidian genera of Pachyderma to be

$$\text{Elephas, Inc. } \frac{2}{0}, \text{ can. } \frac{0}{0}, \text{ mol. } \frac{8-8}{8-8}, = 34.$$

$$\text{Mastodon, Inc. } \frac{2}{0}, \text{ can. } \frac{0}{0}, \text{ mol. } \frac{6-6}{6-6}, = 26.$$

$$\text{Tetracaulodon, Inc. } \frac{2}{2}, \text{ can. } \frac{0}{0}, \text{ mol. } \frac{6-6}{6-6}, = 28.$$

$$\text{Deinotherium, Inc. } \frac{0}{2}, \text{ can. } \frac{0}{0}, \text{ mol. } \frac{5-5}{5-5}, = 22.$$

For the determination of the dental formulæ of Mastodon and Tetracaulodon, Dr. Grant relied entirely on the splendid collection of jaws, crania, and teeth in Mr. Koch's possession, which afford ample means for the solution of that problem. For the dental formula of Deinotherium he has been indebted solely to the casts and fragments of that genus in the British Museum. After explaining the uncertainties and fallacies to which naturalists have been exposed in the identification of species, from not having ascertained the entire dental series in any Mastodon, the sixth section of the memoir describes the distinctive characters and the distribution of the *Mastodon angustidens, M. latidens, M. Elephantoides, M. minutum, M. Tapiroides, M. Andium, M. Borsoni, M. Humboldtii, M. Turicense, M. Avernense, M. giganteum, M. Cuvieri,* and *M. Jeffersoni.* The seventh section of the memoir is devoted to the examination and description of the generic characters of Tetracaulodon, as established by Dr. Godman, and as founded on the number and form of the teeth, the peculiarities of their microscopic structure, the form of the jaws, the tusks, the alveoli of the tusks, the intermaxillary fossa, the infra-orbitary foramina, and other influential characters. The eighth and last section of this paper is occupied with an account of the distinctive characters and the distribution of the known species of this genus; viz. *Tetracaulodon Godmani, T. Collinsii, T. Tapiroides, T. Kochii, T. Haysii,* and *T. Bucklandi.*

June 29, 1842.—Seven communications were read.

1. "Notices connected with the Geology of the Island of Rhodes." By Mr. T. A. B. Spratt, Assistant-Surveyor of H.M.S. Beacon. Communicated by C. Stokes, Esq., F.G.S.

The observations detailed in this paper were made during the summer of 1840. The geological structure of the Island of Rhodes, Mr. Spratt states, is simple, and the distribution of the deposits easily defined. The formations consist of mica schist, shales, limestones, trachyte with basaltic rocks, large beds of shingle, both anterior and posterior in origin to the volcanic æra; and very extensive tertiary deposits.

The mica schists occur in the central districts near Alleyermah and Sclipio, but do not form ridges of very great altitude.

The limestones are scattered in detached masses and rest apparently on argillaceous shales of a black, light cream or reddish colour, but the positive order of superposition the author had no opportunity of determining. He failed also in detecting in them any organic remains, but he is of opinion that they are of contemporaneous origin with the strata near Smyrna, assigned by Mr. Strickland to the Hippurite limestone. The shales are well developed in several places around the base of Mount Ottayaro, but more particularly in the valley west of the village of Embono.

Both the schists and the limestones exhibit, Mr. Spratt states, proofs of great dislocations, and he is inclined to ascribe these effects to the outburst of the volcanic rocks which constitute so large a portion of the central and southern districts of the island. He mentions as instances of these disturbed beds a thin stratum of limestone which

projects, near Lardose, from the enclosing schists like a wall, and traverses several valleys as well as ridges; also some curiously contorted strata on the north face of Mount Agramitty.

The loftiest summits in the island are composed of limestone. Mount Attayaro (anc. Atabyrius), the highest, exceeds 4000 feet in altitude, and at least three-fourths of it are composed of horizontal beds of limestone. The other principal calcareous mountains are Elias, Agramitty, Archangilo and Lindo, all remarkable detached points, and believed by the author to have been islands during the deposition of the tertiary formations.

Mr. Spratt likewise mentions in proof of the limestone mountains forming islands during the tertiary epoch, that at Mount Gallatah, near the north-east extremity of the island, fragments of the rock are honeycombed and perforated exactly in the same manner as the limestone on the shore of many parts of Asia Minor, being the operation of a very minute boring animal.

The igneous rocks constitute the ridges next in altitude, as the lesser Elias and the southern mount of Skathee, besides a great portion of the ridge connecting it with Attayaro and a few others.

The tertiary deposits are assigned by Mr. Spratt to a period posterior to the outburst of the igneous rocks, and when only the higher ranges of hills were above the sea level. They consist of sands and marls tranquilly accumulated in horizontal beds, and are distributed in basins which occupy nearly a third of the island; but having been extensively denudated, they are intersected by deep and wide valleys. The western basins are distinguished from the eastern by containing only freshwater remains. In the hill to the west of Kalavorda the author obtained similar testacea, marine shells being also apparently wanting, but his examination of it was limited. In some of the neighbouring ridges similar strata are also considered to be destitute of organic remains.

No river now flows through the district containing the freshwater deposits, except a small stream about the size of the Bournarbashi of the Troad, nevertheless broad shingle beds traverse the longer valleys and form a remarkable feature in the western division of the island. Mr. Spratt is of opinion that these valleys were the channels of very considerable streams which once flowed from the mountains, and that the accumulations are too great to be accounted for by the torrents of the present winters.

The eastern tertiary deposits contain only marine remains, but in vast abundance in some localities, as in the basins of Lardose, Archangilo, and Koskinou, which the author says, appear to have been inlets or channels protected by the high peaks around the base of which the deposits now lie in horizontal terraces or zones. At Lardose the fossils are most numerous in an insulated hillock of loose sand behind the village, and Mr. Spratt procured there specimens of almost every species which he obtained elsewhere. A quarter of a mile to the northward he noticed a bed of gigantic oysters and "scollops"; the diameter of one of the largest being thirteen inches, and the thickness of one of its valves five inches.

Near Melona and Archangilo fossils may be procured in abundance, but the species are grouped; and about a mile north of the latter place the author found on the end of a low ridge which projected into the plain, a thin stratum of calcareous sand containing numerous fossil leaves, also marine shells and an ichthyolite. The leaves resembled those of the olive, oleander, and plane tree, now growing on the island.

In the neighbourhood of Koskinou and Rhodes fossils are also very abundant, especially in the upper deposits. Mr. Spratt gives the following list of the strata exhibited in a hill near the town of Rhodes, and he says that it affords a type of the whole of the adjacent deposits, with the exception of the distribution of the fossils, which are sometimes wanting, sometimes plentiful, in the same bed.

Top.—Calcareous conglomerate, containing *Turbo rugosa* in great abundance.

Laminated marls in which fossils are sometimes numerous, but at this locality they are wanting.

Coarse sand, inclosing species of Pecten, Turbo, Echini, and corals in great confusion and seldom perfect.

Fine sand from which the author procured only a species of Venus.

Marls without fossils, at this point sometimes indurated.

Greenish sand.

Fine brownish sand with numerous fossils.

Total thickness about 300 feet.

In the deposits along the north shore Mr. Spratt procured no fossils, though he very closely examined Mount Paradiso and Philielmo. The strata in these hills and in that overhanging Tholo and Soronee dip at a considerable angle to the north; and exhibit the greatest visible thickness of the tertiary deposits, Paradiso, the highest, having an altitude of 920 feet; but in the basin of Archangilo they attain nearly the same vertical dimensions.

The tertiary strata are apparently continuous along the north coast, so that no defined margin between the supposed western lacustrine deposits and those of decidedly marine origin is indicated by an intervening ridge or formation of a different character. The long ridge of Skathee is considered however by the author a natural boundary between the basin of Palntshah and the eastern deposits, but he was unable to determine if the strata around Katavyah with which it is believed to be connected, contain marine or freshwater shells.

There are also in several parts of the island elevated shingle beds of considerable thickness; some of them, composed entirely of rounded limestone pebbles, occurring on the sides of the calcareous mountains; while others consist of limestone and volcanic materials, and others again wholly of volcanic fragments. These accumulations, Mr. Spratt says, are evidently of two epochs, one anterior to the great volcanic æra, and the other intermediate between it and the tertiary series, the sands and marls of that group being in several places around them.

2. "On the minute Structure of the Tusks of extinct Mastodontoid Animals." By Alexander Nasmyth, Esq., F.G.S.

The author, at the commencement of his memoir, acknowledges his obligations to Dr. Grant for having first called his attention to the minute anatomical structure of the tusks of Mastodontoid animals; and for having placed at his disposal a copy of the Swedish edition of Retzius's demonstration of the typical structure of the dental organs of animals.

Availing himself of the nble tuition afforded by the Swedish Professor, Mr. Nasmyth says, he has prosecuted the subject, and that these inquiries, besides explaining to him the structure of that portion not completely investigated by Retzius, have unfolded to him some observations which are now generally acknowledged to be truths in the valuable but intricate department of animal development. He further says, that he has been led to results differing somewhat from those of Retzius, so far as the physiology of the cellular tissues is concerned; yet the general appearances exhibited and the manner of displaying them will remain, he adds, lasting memorials of the talents and ingenuity of the Swedish Professor.

The specimens to which Mr. Nasmyth's attention has been directed form part of the collection of Mr. Koch, and they were delivered to him as belonging to *Mastodon giganteum*, *Tetracaulodon Godmani*, *T. Kochii*, *T. Tapiroides*, and the *Missourium*. In the analysis of each specimen he considers—

1st. The constituent structures of the tusk.

2nd. The comparative extent of each of the constituent structures, as far as it can be ascertained.

3rd. Each constituent structure regarded separately in its minute and individual elements.

4th. The conclusions derived from the premises as to the place which the animal should occupy in zoological classifications.

The principle upon which this mode of analysis is based, is that of the infinite variety which nature effects from limited materials, while the constancy of each variety throughout the same species is perfect. This constancy extends, Mr. Nasmyth observes, not only to the constituent structures of each tooth, but to the extent of each constituent, as well as to the peculiar arrangement of the minute elements of which each of these structures is composed.

The examination of each tusk evinces so marked and peculiar a structure, that a cursory inspection will, the author thinks, sufficiently demonstrate specific distinctions, which he supposes must have been accompanied by concomitant peculiarities of organization subservient to separate and distinct habits.

In the following descriptions the word *corpuscule* is used to designate those appearances constituting the characteristic of bone, but denominated by Retzius *cells*, because the author is persuaded that those appearances are truly of a corpuscular character; and the word *cell* is used to designate the structure of the interfibrous material which was left almost entirely out of account by Retzius, and described by others as structureless, but demonstrated by the author to

be most characteristically organized in the different groups of animals. The term *fibres* is used, moreover, to define those appearances which Retzius considers due to a tubular structure, because the author has been unable to find anything which confirms this theoretical appellation founded on the existence of a series of continuous ramifying tubes. This question therefore he leaves in abeyance.

Mastodon giganteum.—The constituent structures of the upper tusks are only two, *crusta petrosa* and *ivory*. The crusta petrosa, in the specimens examined, is comparatively thin, or about half a line; but the extent of the investigation being necessarily limited, the author considers that the observations on this head are incomplete.

The corpuscules of the *crusta petrosa* are scattered irregularly; but they are numerous and give off radiating branched fibres, tending generally either from the surface or to the surface of the tusk. There are hardly any independent fibres. The cellular structure of the interspaces is clearly marked.

The junction of the *ivory* with the *crusta petrosa* is well defined by a clear line, succeeded by a plumose appearance arising from a congeries of very minute ramifying fibres. This appearance looks, Mr. Nasmyth says, as if it arose out of, and formed the termination of, the main fibres which join the layer undivided.

The compartments of which the main fibres are made up are parallelograms resembling those of the Elephant, and are most easily observed in vertical sections, while the cellular structure of the interfibral spaces is clearest in transverse sections. Minute corpuscular appearances are scattered over the substance, and so aggregated as to form at intervals concentric layers. The characteristic differences between the structure of the tusks of the Elephant and Mastodon, Mr. Nasmyth observes, consist principally in the presence of transverse fibres in the *crusta petrosa* of the Elephant, and the greater number and regularity of its corpuscules in the Mastodon, as well as in the peculiar disposition to a transverse direction of its radiating fibres. In the ivory the most striking peculiarity consists in the numerous bands of corpuscular-looking bodies in its substance. These appearances, so frequently observed in ivory, Mr. Nasmyth is of opinion, depend, as pointed out by him, on the thickness of the animal matter of the interfibral cells.

Tetracaulodon Godmani.—The author says there is a great dissimilarity in the constituent structures of tusks of this Pachyderm and those of the Mastodon, while on a cursory examination of the minute organization of these structures there is an apparent similarity. The crown of both the upper and under tusk is coated with enamel extending below the level of the alveolar process, with *crusta petrosa* external to it, the body of the tusk being composed of ivory. The alveolar process of the upper tusks is large and deep, greatly exceeding that of every other tusk which the author has examined, and showing, he says, that the actions in which these organs assisted, must have been very powerful.

The habits essentially necessary to the exigencies of an animal being, Mr. Nasmyth observes, the same in youth as in adult age, the

organization of the individual tissues is the same at both periods, though certain modifications of instruments are exacted at successive stages of existence. Thus, in early youth, when the frame is not powerful, every efficiency is given to the cutting edges of the dental apparatus; and the author states a fact he believes never before remarked, though long noticed by himself, that the tusks of the young Elephant and Walrus are tipped with a very thin layer of enamel.

The head of the *Tetracaulodon Godmani* examined by Mr. Nasmyth is shown to have been that of an animal in which two of the adolescent teeth are well developed. The crusta petrosa of the tusk was about half a line thick, and extended over the whole of the visible surface. The corpuscules were irregularly disposed, but closely aggregated, and exhibited in the transverse section an irregularly circular shape with occasionally angular points. The radiating fibres were numerous, ranging in all directions; and the independent transverse fibres were also numerous, traversing with a curved course the whole substance. The cells of the interspaces were visible. The enamel on the upper tusk was a line thick. The parallel rows of constituent cells throughout the external half ranged in straight lines, but throughout the internal half they were curved diagonally. There was no clear space between the enamel and ivory, but the line of junction was well defined. A plumose layer of fibres, apparently the peripheral termination of the main undivided fibres of the ivory, succeeded to the enamel. The component bulbs of the fibre were round, but not often visible, and were best seen in the longitudinal section. The fibres were placed at about the distance of two interfibral spaces, and curved in the transverse section as well as in the vertical, but in the latter direction slightly. A minute corpuscular appearance was scattered over the substance, and the cells of the interfibral material were visible.

The crusta petrosa, enamel and ivory of the under tusk were similar to those of the upper, except that the constituents were so transparent as hardly to betray any characteristic. The parietes of the cells of the enamel are more defined in the under tusk.

Besides the important characteristic of the thick coating of enamel, the tusk of the *T. Godmani* presents manifest differences from that of the other species, in the elements of each of the constituents. The radiating fibres of the corpuscules differ from those of *Mastodon giganteum* in being given off equally in all directions: in the *M. giganteum* the numerous independent fibres of the *T. Godmani* are also absent, and the zones or belts of minute corpuscules in the ivory of the *M. giganteum* are wanting in that of the *T. Godmani*.

Tetracaulodon Kochii.—The tusks of this Pachyderm have only two constituents, *crusta petrosa* and *ivory*. The crusta petrosa varies in thickness, equalling in some parts an inch. In the vertical section the corpuscules are irregularly oval and irregularly disposed at the distance of three or four corpuscular diameters, and they give off occasionally many fine radiating fibres. Numerous independent transverse fibres pass in a curved direction also throughout the substance, their beaded or minute corpuscular appearance being very visible,

and they are of an irregularly twisted oval form. The cells of the interspaces are likewise visible.

The ivory of the upper tusks consists of very slightly undulating, undivided fibres, with the cells of the interfibrous substance well marked, but semi-transparent. The fibres of the under tusk slightly undulate, and present occasionally an appearance of thorny projections. The compartments of the fibres are easily seen, and are irregular in size, but rounded.

Tetracaulodon Tapiroides.—The tusks consist also of only *crusta petrosa* and *ivory*, and the resemblance in the microscopic structure of this species with that of *T. Kochii* is great. The thickness of the crusta petrosa is considerable. The very irregularly-shaped corpuscules, placed at intervals of two or three corpuscular diameters, are semi-transparent, and without radiating fibres in the external half; but those situated in the internal half are of the usual opacity, and give off numerous radiating fibres. Transverse, irregularly beaded, independent fibres traverse the substance, making one distinct curve in their passage across it. The cells of the interspaces are slightly visible.

The ivory is so translucent and homogeneous as to exhibit generally very little character. The fibres undulate but do not divide, forming an abrupt line of junction with the crusta petrosa. The form of the beaded compartments of the fibre is oblong, not rounded, as in *T. Kochii*, and they do not exhibit thorny projections. These are the only marked differences in the two species.

The cells of the semi-transparent interfibral space are generally visible.

Missourium.—The constituents of the tusks are likewise *crusta petrosa* and *ivory*; but their intimate structure, Mr. Nasmyth says, is more peculiar, so far as his examination has extended, than that of the tusks of the preceding animals.

The crusta petrosa, in the section which the author was permitted to make, was more than three-eighths of an inch thick. The corpuscules were very numerous, and generally within the distance of one diameter. The granulated compartments of which the corpuscules were composed, were very visible, and often without radiating fibres, but where these occurred they were of a coarse structure. The transverse independent fibres were beaded in coarse, somewhat tortuous, ovoid compartments, and ranged very close to one another, with interfibral spaces of about only two fibral diameters, and followed a straight, perpendicular and parallel course to the surface. The cells of the semi-transparent interfibral space were generally visible.

The appearances presented by the ivory at its junction with the crusta petrosa, Mr. Nasmyth was unable to ascertain; but in the substance of the ivory the fibres undulated, and their beaded compartments had a rounded shape: these fibres were frequently invested with an irregular congeries of granules distinct from the interfibral cells. Towards the central portion of the ivory the compartments forming the fibre were frequently so disposed as to give the fibre a peculiar tortuous appearance.

The peculiarities of the tusk of the Missourium are given by Mr.

Nasmyth as follows; and, he says, they would certainly indicate a distinct species of Mastodontoid animal:—

1. The great extent of the crusta petrosa. 2. The close aggregation of its corpuscules. 3. The granulated structure of these corpuscules. 4. The coarse granulated structure of the compartments of the radiating fibres. 5. The close parallel perpendicular arrangement of the fibres of the crusta petrosa. 6. The irregular congeries of granules surrounding the fibres of the ivory. 7. The peculiar tortuous appearance occasionally exhibited by these fibres.

On the whole, Mr. Nasmyth observes, the several species of animals noticed in his paper seem to be nearly allied, and fitted to exist under nearly similar conditions; and though the early æras to which these Pachyderms must be referred, present, he says, considerable uniformity of circumstance, yet they must have demanded some variety of detail in the animal organization.

Finally, the characteristics in the minute structure of the tusks of all the five animals betray, the author observes, greater varieties than are found to exist even betwixt some genera possessed of tusks; and if it be established that specific differences positively do exist among all these animals, then the value of this kind of observation is great; but if the five animals are all to be grouped in one category, then this mode of observation is of no value in palæontological researches.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from page 314.]

June 9. (Communications respecting the Comet continued.)—15. Letter from Professor Kendall, containing Observations of the Comet made at Philadelphia. Communicated by Lieut.-Col. Sabine.

Philadelphia, April 27, 1843.

Sir,—I send you the result of the observations of the great comet of February 1843, made by Mr. Walker and myself with the Fraunhofer equatoreal, at the Observatory of the Central High School, latitude 39° 57′ 8″, longitude $5^h\ 0^m\ 41^s{\cdot}9$ west of Greenwich. The measures were all made with the Fraunhofer filarmicrometer, power 75, except on the 9th and 10th of April, when the extreme faintness of the comet compelled us to use the ring-micrometer. We first saw the nucleus on the 11th of March and brought the comet to the centre of the field, and read the graduations. The place given on that evening is liable to an error of two minutes of space. That of the 10th of April is liable to an error of about one minute of space. Those of the other evenings were the result of satisfactory measures. The nucleus on the 11th of March was near the star ζ Ceti, of the third magnitude, and was of about the same brightness. The tail extended between Rigel and Sirius, about 1° south of its position on the 18th, when we saw it and also the nucleus, but made no measures. In the comet-searcher the nucleus appeared on the 11th, with a well-defined disc, larger than that of Jupiter in the same instrument. In the 9-feet equatoreal it had no appearance of a disc, but only of a nebulosity gradually condensed toward the centre; so that it was impossible to distinguish any nucleus. I have no doubt that this comet was seen in the day-time, on the 28th of February

and the 1st of March. The particulars are stated at length in Professor Silliman's Journal. An observer at Woodstock, Vermont, saw the nucleus and tail in a good telescope, probably a 3½-feet Dollond. Mr. Clark of Portland-Maine, a teacher of navigation, measured its distance from the sun's limb at the time of culmination, and found it to be 6° 15¼′. Professor Loomis, of Western Reserve College, Hudson, Ohio, has computed the intensity of the comet's light on the 28th of February, and finds it to have been twenty-four times brighter than on the 11th of March; that is, twenty-four times brighter than a star of the third magnitude.

Mean Time, Philadelphia.	Star of Comparison and Magnitude.	No. of Measures.	Comet's observed R. A. corrected for Parallax, but not for Aberration.	No. of Measures.	Comet's observed Dec. corrected for Parallax, but not for Aberration.
1843. d h m s			h m s		° ′ ″
March 11 7 21 20·79	....	......	1 43 35·00	......	−11 35 23·00
19 7 25 55·68	*b, 7·8	2	2 57 14·46	2	− 9 26 50·44
	*c, 9·10			1	− 9 26 53·02
22 7 46 48·46	*h, 8·9	7	3 17 44·47	2	− 8 35 58·55
23 7 39 59·79	*i, 8	14	3 23 50·21	3	− 8 19 13·16
24 7 26 51·79	*k, 8·9	4	3 29 36·44	2	− 8 3 35·48
	*l, 8·9	4	36·61	1	40·35
	*m, 8	4	36·74	1	54·90
26 7 36 10·32	*n, 8·9	5	3 40 28·93	1	− 7 32 27·12
	*o, 8	5	29·59	1	− 7 32 17·14
April 1 7 0 20·88	*s, 9	8	4 7 54·53		
	*t, 8·9	6	54·68		
2 7 48 6·35	*u, 9	8	4 11 50·91	2	− 5 58 46·84
7 7 52 10·20	*v, 8	4	4 29 33·93		
	*y, 8	4	33·93		
	*z, 9	4	33·44		
9 7 57 59·64	*A, 9	7	4 35 52·21	7	− 4 45 38·29
10 8 21 46·25	*A, 9	1	4 39 1·00	1	− 4 36 38·00

Apparent places of the Stars compared above with the Comet.

Name.	Right Ascension.	Declination.
	h m s	° ′ ″
b	2 57 37·68	−9 33 31·84
c	2 57 47·57	−9 27 27·04
h	3 19 17·08	−8 32 6·08
i	3 24 25·03	8 22 38·02
k	3 30 12·86	8 10 56·86
l	3 30 17·24	8 10 4·51
m	3 30 31·57	8 0 13·08
n	3 40 9·48	7 29 53·63
o	3 41 8·18	7 30 2·58
s	4 8 15·74	
t	4 8 57·61	
u	4 13 21·22	5 56 13·37
v	4 27 36·41	5 3 46·09
y	4 31 21·13	5 7 25·53
z	4 31 24·05	5 0 48·52
A	4 36 4·31	4 35 56·58

From the observations of the 19th and 26th of March, and 2nd of April, we have computed the following elements:—

Perihelion Passage, Feb. $27^{d}\cdot436953$, Greenwich mean time.

Long. of Ascending Node....	$1^\circ 55' 18''\cdot6$	from mean equinox of [March 26.
Long. of Perihelion	277 43 53·7	... ...
Inclination	35 34 0·8	
Perihelion Distance	0·00701906	$\log q = 7\cdot8462789$

Motion retrograde.

The ephemeris computed from these elements, after applying aberration, requires the following corrections in order to agree with our observations:—

	Correction in Right Ascension $\frac{1}{15}\,\Delta\,\alpha$.	Correction in Declination $\Delta\,\delta$.
March 19.	$-0^{s}\cdot33$	$+38''\cdot3$
22.	$-1\cdot19$	$+\ 5\cdot0$
23.	$-1\cdot38$	$+27\cdot5$
24.	$-0\cdot94$	$+\ 1\cdot8$
26.	$-2\cdot07$	$+23\cdot9$
April 1.	$+1\cdot99$	
2.	$+4\cdot50$	$-21\cdot6$
7.	$+7\cdot82$	
9.	$+9\cdot25$	$-54\cdot1$

This corresponds well enough with the observations to be used in computing the parallax and aberration, and in reducing to a common date the places observed during the same half hour. These elements have some resemblance to those of the comet of 1689 as computed by Pingré. The inclination, however, of the latter, $69^\circ 17'$, differs too much to be consistent with their identity. Professor Benjamin Pierce, of Harvard University, Cambridge, Mass., has recomputed the observations used by Pingré, and finds for the elements of the comet of 1689,

Perihelion Passage 1689, December $2^{d}\cdot1403$, Greenwich mean time.

Longitude of Ascending Node....	$344^\circ\ 18'$
Longitude of Perihelion	271 16
Inclination	30 25
Perihelion Distance	0·0103

Motion retrograde.

The elements of the comet of 1843, with a period from 1689, December $2^{d}\cdot1403$, to 1843, February $27^{d}\cdot4370$, represent the places given by Pingré within 5°. Whether the errors of Pingré's places of the comet of 1689, together with the effect of perturbations, amount to 5°, is a subject worthy of investigation. It has never happened, I believe, that two comets have appeared with elements agreeing so well, without being found in the end to be the same.

Respectfully,

Lieut.-Colonel Sabine, R.A., Woolwich. E. O. Kendall.

16. Observations of Distance of the Comet from known stars, made at Demerara by Captain Geale of the ship Isabella, Lieutenant A. S. Glascott, R.N., and James Donald, Esq. Communicated by Sir John Herschel.

March 11 at $7^h\ 20^m$ M.T.	Distance of comet from	Rigel	= 50° 23′
		Sirius......	= 71 21
		Capella ...	= 77 30
		Canopus	= 68 23
	Length of tail 46°.		
March 12 at 7 22	Distance from	Rigel	= 47 24
		Sirius	= 69 0
		Aldebaran	= 46 40
		Canopus	= 67 11
		Capella ...	= 71 55
March 13 at 7 5	Distance from............	Canopus...	= 65 1
		Capella ...	= 70 13
		Sirius	= 66 36
		Aldebaran	= 43 30
March 19 at 7 15	Distance from............	Capella ...	= 62 24
		Sirius	= 54 14
		Canopus..	= 59 17
		Rigel	= 32 6
	Length of tail 42°.		
March 26 at 7 10	Distance from............	Sirius	= 44 10
		Capella ...	= 56 55
		Canopus...	= 55 46
		Rigel	= 21 35
	Length of tail 32°.		
March 27 at 7 12	Distance from	Sirius	= 43 8
		Rigel	= 20 30
		Canopus...	= 55 37
		Capella ...	= 56 28
	Length of tail 30°.		
March 31 at 7 35	Distance from............	Canopus...	= 53 28
		Sirius......	= 58 31
		Capella ...	= 54 41
		Aldebaran	= 23 30
	Length of tail 24°.		

17. Some Account of the Comet, in a Letter from J. Gimblett, Esq. Communicated by Sir John Herschel.

18. Extract of a Letter from Lieut.-Colonel Harvey, 14th Light Dragoons, dated Poona, March 13. Communicated by Professor Narrien.

LVIII. *Intelligence and Miscellaneous Articles.*

ON THE COMPOSITION OF PECHBLENDE. BY M. EBELMAN.

THE author remarks that the uranium in this mineral has hitherto been regarded as identical with the olive-green oxide of uranium, which before the experiments of M. Peligot was considered to be the protoxide; but it is to be remarked, M. Ebelman observes, that pechblende, even when reduced to very fine powder, retains its deep black colour, as it also does when heated in a current of azote to deprive it of water, whereas if heated to redness in atmospheric air

it immediately becomes olive-green, and hence it may be concluded that pechblende is not identical with the olive-green oxide.

The pechblende from Joachimstal in Bohemia, when treated with hydrochloric acid, yields at first carbonic acid gas, afterwards hydrosulphuric acid, and eventually dissolves almost entirely, leaving only a little gelatinous silica, entirely soluble in potash. The filtered solution was found to contain lead, iron, manganese, lime, magnesia, and a small quantity of soda. When pechblende is heated in a current of dry chlorine it yields only chloride of sulphur. Pechblende acted on by chlorine is partly soluble in water, and leaves a yellow residue of urinate of lime and magnesia; the solution contains neither antimony, bismuth, arsenic, copper, or zinc.

To analyse pechblende it was treated with nitric acid; the silica was separated by evaporating the solution to dryness; the residue was treated with hydrochloric acid and filtered. The lead was separated by hydrosulphuric acid, converted into sulphate and estimated; to the solution after the separation of the lead hydrosulphate of ammonia was added, which precipitated the uranium, iron and manganese; the method by which these three metals were separated will be presently stated. The solution from which they were precipitated was boiled, then treated with oxalate of ammonia, the oxalate of lime converted into sulphate and estimated. The filtered solution was evaporated to dryness, and the residue heated to redness to expel the ammoniacal salts, sulphuric acid was then added, and the sulphates of magnesia and soda were obtained; the alkali was separated from the magnesia by means of acetate of barytes.

The sulphur was determined by a separate experiment, and its quantity was exactly proportional to that of the lead; the carbonic acid was expelled by nitric acid, and its quantity determined by that of the carbonate which it precipitated from barytes water.

The water is readily separated by heat; it was obtained by heating the pechblende in azotic gas, and absorbing it by chloride of calcium.

The uranium was separated from the iron and manganese by the following means:—the solution of carbonate of uranium in carbonate of ammonia is not rendered turbid by the addition of hydrosulphate of ammonia; and this fact, which has not been before noticed, allows of the separation of uranium from several metallic oxides slightly soluble in carbonate of ammonia, such as those of manganese, cobalt, nickel and zinc, which hydrosulphate of ammonia completely precipitates from this solution: this separation of uranium from the above-named oxides is rendered very simple by this process.

In the present case the uranium, manganese and iron having been precipitated by hydrosulphate of ammonia, were redissolved in dilute *aqua regia*, and the liquor supersaturated with carbonate of ammonia, precipitated peroxide of iron mixed with some manganese; hydrosulphate of ammonia added to the filtered liquor separated a little sulphuret of manganese, and it was then boiled till colourless; the precipitate obtained is greenish, owing to the partial reduction of the oxide of uranium by the hydrosulphate; it was obtained in the state of green oxide by calcination; the iron and manganese were separated by succinate of ammonia.

M. Ebelman determined the state of oxidation of the uranium in pechblende, by a modification of a process which he has described in the sixteenth volume of the *Annales des Mines*, and the results of his analyses are—

Black oxide of uranium	75·23
Sulphuret of lead	4·82
Protoxide of iron	3·10
Protoxide of manganese	0·82
Silica	3·48
Lime	5·24
Magnesia	2·07
Soda	0·25
Carbonic acid	3·32
Water	1·85
	100·18

Ann. de Ch. et de Phys., Août 1843.

ON THE COMPOSITION OF WOLFRAM. BY M. EBELMAN.

Until lately wolfram has been considered as a compound of tungstic acid with the protoxides of iron and manganese; but recently, M. Schaffgotsch (*Ann. de Ch. et de Phys.* ii. p. 532) has stated that it contains the oxide of tungsten and not the acid. He has deduced this from the results of his analyses, which all gave an excess of five or six hundredths when the tungsten was estimated as tungstic acid. M. Wöhler arrived at the same conclusion from the action of chlorine on wolfram.

M. Ebelman remarks, that an experiment which is easy of execution appeared to him to be sufficient to decide the question: wolfram is acted upon by hydrochloric acid when boiling, and leaves a residue which is evidently tungstic acid.

The mean of five experiments on wolfram from the environs of Limoges gave the following results:—

Tungstic acid	76·20
Protoxide of iron	19·19
Protoxide of manganese	4·48
Magnesia	0·80
	100·67

The mean of two experiments made upon fragments of a large crystal of wolfram from Zinnwald, gave

Tungstic acid	75·99
Protoxide of iron	9·62
Protoxide of manganese	13·96
Lime	0·48
	100·05

Ann. de Ch. et de Phys., Août 1843.

ON THE PRODUCTS OF THE DECOMPOSITION OF AMBER BY HEAT. BY MM. PELLETIER AND PHILIPPE WALTER.

The authors remark, that the phænomena of the distillation of amber have been observed with the greatest attention by MM. Robi-

quet and Colin (*Ann. de Chem. et de Phys.*, tom. iv. p. 326); they state, that when amber is heated in a glass retort it softens, fuses, swells up considerably and yields succinic acid, oil and combustible gases; as the production of the acid proceeds the swelling up diminishes, and soon ceases altogether. If the fused matter be now examined, it is found to possess an even fracture of a vitreous and resinous aspect; if, on the contrary, it be heated quickly, it boils rapidly without swelling, and produces so large a quantity of oil that it flows in small streams; lastly, when the matter appears to be so completely carbonized that it yields scarcely any oil, and the retort be then heated till it softens, a yellow substance sublimes which has the consistence of wax.

If this waxy matter be treated with cold æther the *micaceous* matter of MM. Robiquet and Colin is obtained, but if it be boiled in absolute alcohol, taking care not to use enough to dissolve the whole mass, it will be observed that the portion which does not dissolve is of a much deeper yellow, less micaceous and more pulverulent than the original substance; it will also be seen that the first portions which crystallize, either by the evaporation or cooling of the æther, is of a much less intense yellow than the original matter; lastly, by the almost complete evaporation of the æther, a crystalline matter of a still less deep colour is obtained.

When each of these three products is separately treated with alcohol they behave in the same manner; a very yellow substance which does not dissolve, a less yellow substance which crystallizes first, a still paler substance remaining in the mother-water.

Eventually, however, after numerous experiments, the authors obtained only two substances; one in a very small quantity: this was pulverulent, scarcely crystalline, of a fine yellow colour, insoluble in cold alcohol, and scarcely soluble in it or æther when boiling; the other substance is white, in very fine flattened acicular crystals, more soluble in alcohol and in æther. This last is the true peculiar crystalline substance which constitutes the pyrogenous wax of amber; it is in quantity to the yellow matter insoluble in alcohol as 90 to 10.

The authors then state, that by various modes of treatment with alcohol of different strengths and æther, they obtained from heated amber,—1st, oil; 2ndly, yellow substance; 3rdly, white crystalline matter; 4thly, a brown bituminous matter, very soluble in alcohol, and possessing the characters of the non-acid pyretin of Berzelius.

Yellow substance.—The properties of this are that it is insoluble in water, scarcely soluble in boiling alcohol or æther; it is rather pulverulent than crystalline; requires a temperature of 464° Fahr. to melt it; it then volatilizes, and the greater part of it is decomposed. When heated in nitric acid it is converted into a reddish yellow resinous matter. Cold sulphuric acid has no sensible action upon it; when heated it dissolves it, acquiring a deep blue colour with a shade of green. By analysis it yielded

Hydrogen	5·8
Carbon	94·4
	100·2

This analysis, and the properties of this substance, prove its identity with that which M. Laurent calls *chrysène.*

White crystalline substance.—This is inodorous, insipid, scarcely soluble in cold alcohol, very sparingly soluble in æther, but more so than the preceding substance; soluble [fusible?] at 320° Fahr.; when heated in close vessels to above 576° Fahr. it is volatilized, a small portion, however, is decomposed with a small residue of charcoal; it dissolves in the fixed and volatile oils, but the alkalies do not act upon it. The mineral acids when cold do not attack it; when heated, sulphuric acid dissolves it, and assumes a deep blue colour without any shade of green, and it is soon carbonized. If before this effect is produced the acid be diluted, it becomes colourless, but recovers its colour by concentration; by hot nitric acid it is converted into a resinous matter. By analysis it yielded

	I.	II.	III.
Hydrogen	5·6	5·8	5·5
Carbon	95·6	95·3	95·8
	101·2	101·1	101·3

From these results the authors are of opinion that this substance is not merely isomeric, but identical with the *idrialine* of M. Dumas; and they propose to call it *succistérène.*—*Ann. de Ch. et de Phys.* ix. 89.

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1843.

Chiswick.—October 1. Fine: clear: overcast. 2. Overcast: showery. 3, 4. Cloudy and mild. 5. Very fine. 6. Densely clouded: rain. 7. Cloudy: rain. 8. Boisterous: overcast. 9. Rain. 10. Clear: overcast: rain. 11. Boisterous: heavy rain. 12. Boisterous: rain. 13, 14. Clear: cloudy and fine. 15. Foggy: cloudy: frosty and foggy. 16. Frosty: clear and cold: frosty. 17. Stormy, with rain. 18. Cloudless: clear and frosty. 19. Frosty haze: clear: frosty. 20. Frosty haze: fine: cloudy. 21. Cloudy: showery: clear. 22. Cloudy and fine: stormy at night. 23. Clear: cloudy: clear. 24. Densely clouded. 25. Cloudy: clear. 26. Frosty: very fine: clear. 27. Very fine: boisterous, with rain at night. 28. Boisterous: clear and fine. 29. Hazy: clear: foggy. 30. Hazy: rain. 31. Heavy rain.—Mean temperature of the month 2½° below the average.

Boston.—Oct. 1, 2. Cloudy: rain early A.M. 3. Fine. 4, 5. Cloudy. 6. Cloudy: rain P.M. 7. Fine. 8. Cloudy: rain early A.M. 9. Rain: rain early A.M.: rain A.M. 10. Fine. 11. Rain. 12. Rain and stormy. 13. Fine. 14. Windy: ice this morning. 15, 16. Fine. 17. Cloudy: rain early A.M.: stormy night, with rain. 18—20. Fine. 21. Cloudy: rain early A.M. 22. Cloudy: rain P.M. 23. Fine. 24. Fine: rain P.M. 25—27. Fine. 28. Stormy: rain early A.M. 29. Fine. 30. Cloudy: rain early A.M.: rain P.M. 31. Cloudy.

Sandwick Manse, Orkney.—Oct. 1. Showers. 2. Showers: clear. 3. Showers: large hail. 4. Rain. 5. Drizzle. 6. Rain: showers. 7. Bright: showers. 8, 9. Cloudy: clear. 10. Showers. 11. Frost: showers. 12. Showers: hail. 13. Large hail. 14. Bright: showers. 15, 16. Hail-showers. 17. Snow-showers: clear frost. 18. Clear frost: showers. 19—21. Showers. 22. Clear frost: showers. 23. Showers. 24. Showers: sleet: showers. 25. Showers. 26. Showers: aurora. 27. Cloudy: rain. 28. Drizzle. 29. Showers. 30. Showers: fine. 31. Showers: fine: clear.

Applegarth Manse, Dumfries-shire.—Oct. 1. Cloudy: rain P.M. 2. Fine. 3. Dull. 4. Cold: dull. 5. Fine: mild. 6. Wet, but mild. 7. Rain. 8. Showers. 9. Clear: fair. 10. Dull: fair. 11. Wet. 12. Cold: snow on the hills. 13. Cold: hail-shower. 14, 15. Fine and clear. 16. Fine: dry. 17. Rain and sleet. 18. Fine: frosty. 19. Clear: fair. 20. Dull: wet P.M. 21. Clear and sunny. 22. Very wet: cleared P.M. 23. Boisterous: showers. 24. Wet. 25, 26. Fine: frost A.M. 27. Fine. 28. Fair: chill. 29. Heavy rain. 30. Fair: frost. 31. Wet: frost A.M.

Meteorological Observations made at the Apartments of the Royal Society, London, *by the Assistant Secretary*, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London*; *by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.								Thermometer.											Wind.					Rain.					Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		London: R.S.			Chiswick.		Boston. 8½ a.m.	Dumf.-shire.		Orkney, Sandwick.		London: R.S. 9 a.m.	Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	London: R.S. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Lond. R.S. 9 a.m.
1843. Oct.		Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Fahr. 9 a.m.	Self-reg. Mx.	Self-reg. Min.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.											
1.	30·048	29·999	29·926	29·35	29·79	29·75	29·40	29·50	65·0	67·0	56·0	72	55	63	64	56	54	53	wnw.	w.	w.	sw.	w.	......	·11	·04	......	·18	57
2.	30·066	30·009	29·999	29·30	29·82	29·95	29·59	29·81	62·3	70·0	56·3	67	47	58	...	51	50	47½	sw.	nw.	w.	sw.	wsw.	......	·01	·05	......	·18	58
3.	30·214	30·138	30·061	29·50	......	29·93	29·75	29·89	58·3	64·8	53·4	65	54	55·5	63	49½	50½	46	w.	sw.	calm	sw.	w.	·119	·01	......	......	·14	55
4.	30·194	30·117	30·091	29·46	29·93	29·93	29·83	29·70	61·3	65·2	56·8	70	50	61	61	49	48½	52	w.	w.	w.	sw.	s.	......	......	......	......	......	57
5.	30·142	30·075	29·895	29·46	29·80	29·75	29·85	29·70	58·7	67·4	58·4	73	48	56·5	62	54	44	45	w.	e.	calm	sw.	e.	......	......	......	......	·60	57
6.	29·792	29·736	29·187	29·14	29·40	29·20	29·22	29·10	61·2	66·4	57·3	65	54	60	55	50	54½	52	s.	sw.	sw.	sw.	s.	......	·19	......	......	·47	58
7.	29·636	29·592	29·440	28·94	29·25	29·16	29·02	29·10	60·5	65·0	57·2	69	57	59	59	52	54	51	sw.	sw.	w.	sw.	w.	·158	·06	·04	......	·11	58
○ 8.	29·434	29·683	29·378	28·73	29·19	29·40	29·20	29·43	62·3	65·8	60·2	62	50	58	54	48	48½	43	sw var	w.	w.	w.	ne.	·016	·22	·18	......	......	59
9.	29·354	29·715	29·282	28·93	29·35	29·65	29·53	29·74	47·8	64·2	49·3	54	35	49	52	38	45	39½	n.	w.	calm	w.	ne.	·263	·11	·17	......	·03	54
10.	29·958	29·916	29·601	29·36	29·52	29·50	29·75	29·62	47·6	54·2	43·0	64	46	46·5	50	40	40	35½	s.	sw.	calm	w.	ne.	·030	·40	·18	......	·13	48
11.	29·238	29·217	29·098	28·76	29·12	28·90	29·44	29·40	59·2	60·5	48·2	60	42	49	47	40	39	40	sw var	sw.	calm	e.	ne.	·352	·14	·24	2·20	·21	53
12.	29·152	29·537	29·090	28·52	29·20	29·48	29·25	29·32	47·5	62·2	45·0	50	32	42·5	47	38	41	36½	w.	nw.	w.	nw.	n.	·161	·09	·11	......	·17	48
13.	29·706	29·753	29·598	29·18	29·45	29·45	29·20	29·54	40·3	50·3	39·2	53	32	39	46	30	37½	39	wnw.	w.	w.	nw.	n.	·080	......	......	......	·39	39
14.	29·760	29·771	29·740	29·30	29·66	29·71	29·69	29·72	42·0	50·2	40·0	54	29	39	50	30	42	37½	wnw.	nw.	w.	nw.	n.	......	......	......	......	·11	40
15.	29·720	29·697	29·649	29·32	29·62	29·65	29·70	29·68	38·7	49·8	38·0	52	25	37·5	46	26	37	36½	wnw.	s.	calm	w.	n.	......	......	......	......	·12	40
☾ 16.	29·692	29·682	29·621	29·31	29·58	29·28	29·51	29·52	38·8	46·0	36·3	49	27	36·5	47	22½	38	33	n.	nw.	calm	nw.	n.	......	·52	......	......	·15	37
17.	29·270	29·377	29·250	28·91	29·38	29·70	29·62	29·74	43·3	45·0	38·6	48	36	40	45	31	37	31½	s.	nw.	sw.	e.	n.	·327	·06	·51	......	·15	40
18.	29·892	30·157	29·855	29·45	29·85	30·10	29·99	30·11	40·6	46·7	38·3	50	24	38	44	28	37	38	w.	nw.	nw.	nw.	n.	·058	......	·44	......	·08	35
19.	30·350	30·343	30·312	29·93	30·24	30·23	30·15	30·00	36·7	47·0	34·6	52	22	35	46	25	44	45	wnw.	w.	calm	sw.	wsw.	......	......	......	......	·08	33
20.	30·338	30·321	30·104	29·84	30·02	29·72	29·89	29·62	37·2	49·6	33·0	57	37	37	51	31	47	47	nnw.	sw.	calm	s.	s.	......	·25	......	......	·28	33
21.	29·894	30·045	29·869	29·36	29·69	29·83	29·57	29·65	49·3	52·4	37·6	56	31	48·5	59	45	48	47	s.	w.	calm	nw.	w.	·213	·01	·24	......	·13	42
22.	30·116	30·086	29·897	29·58	29·65	29·56	29·44	29·38	46·3	54·8	40·7	60	44	45	54	40	50½	49½	s.	sw.	calm	sw.	s.	......	·11	......	0·94	·14	39
● 23.	30·084	30·048	30·012	29·44	29·77	29·63	29·50	29·24	51·0	56·2	46·6	61	47	53	55	45	47	45	w.	sw.	nw.	wnw.	sw.	·075	......	·11	......	·18	46
24.	29·892	29·868	29·544	29·28	29·48	29·35	29·00	29·04	54·4	56·5	49·7	59	45	53·5	45	44	45	41	sw.	sw.	calm	wnw.	w.	......	·31	......	......	·42	47
25.	29·432	29·404	29·391	28·95	29·28	29·29	29·08	29·22	45·7	57·4	46·3	52	28	44	48	30	42½	41	n.	ne.	calm	w.	w.	·352	......	·33	......	·29	47
26.	29·522	29·614	29·479	29·07	29·39	29·50	29·38	29·46	39·3	51·3	38·5	54	26	38	47	29½	42	40½	n.	w.	calm	w.	nw.	......	......	......	......	·53	41
27.	29·734	29·801	29·298	29·25	29·48	29·22	29·42	29·33	39·3	49·8	35·7	55	41	37	45	24½	40	40½	s.	s.	calm	w.	...	......	·17	......	......	......	41
28.	29·128	29·389	29·093	28·49	28·61	28·75	29·30	28·90	45·3	50·2	40·6	53	27	46	49	37	42	46	s.	sw.	w.	e.	e.	·122	......	·17	......	·35	43
29.	29·620	29·602	29·534	29·17	29·18	29·43	28·85	29·20	42·7	49·8	39·8	54	31	41	44	39	45	44	s.	s.	w.	w.	nw.	......	·36	......	......	·44	40
☽ 30.	29·422	29·877	29·305	28·95	29·42	29·20	29·43	29·31	51·8	52·3	43·2	62	46	47·5	43	27½	43	43	se.	s.	e.	sw.	nw.	·161	·25	·21	0·92	·25	44
31.	29·412	29·902	29·394	28·98	29·38	29·49	29·25	29·52	50·0	59·3	50·2	45	42	42·5	46½	30	42	36	n.	sw.	w.	sw.	w.	·322	·88	·33	......	·07	49
Mean.	29·749	29·825	29·612	29·20	29·317	29·538	29·474	29·499	49·2	56·4	45·5	57·96	39·03	46·9	50·8	38·0	44·37	42·64						Sum. 2·890	4·19	3·35	4·16	6·38	Mean. 46

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LIX. *On* Dr. Hare's "*Additional Objections*" *relating to Whirlwind Storms. By* W. C. REDFIELD*.

IN my reply to Dr. Hare's first series of "Objections" and "Strictures," I attempted to show that these could have no weight or efficacy in disproving the whirlwind character of violent storms and tornadoes; and, furthermore, that convincing evidence of whirlwind action in the tornado of New Brunswick was found in those very facts which he had set forth and relied on for disproving its rotation †.

Besides correcting, on that occasion, certain grave errors into which my opponent had fallen, I also referred to the additional proofs of rotation which are found in my published survey of the effects of this tornado ‡. This was deemed sufficiently conclusive in replying to Dr. Hare, who had chosen to "enter the lists" as my assailant and in support of his own and Mr. Espy's notion of the centripetal course of the wind in these storms; for the effects produced by the New Brunswick tornado had been greatly relied on by each of these writers, as proving such centripetal course in the wind of tornadoes.

At that time I possessed, in my field-notes and surveys, abundant evidence of a constant rotative action in several other tornadoes, and plans or diagrams which exhibit portions of this evidence had long been prepared; but I saw no defect in the proofs of rotation previously shown that required their publication.

One of the tornadoes which I had thus prepared to illustrate, was that which passed near Providence in 1838, some account of which had been published by Dr. Hare; and, as partiality for his own electrical hypothesis may have induced him to engage in this controversy, I now refer to a paper con-

* Communicated by the Author.

† This Journal for May 1842, p. 353-369.

‡ For this survey, &c. see this Journal for January 1841, p. 20-29.

taining what I deem decisive evidence of the whirling character of the Providence tornado, published since the first appearance, in America, of his "Additional Objections," and found in this Journal for January 1843, p. 38–52.

It has probably been perceived, that in advancing his "additional objections," which are found in this Journal for August last, Dr. Hare seems virtually to abandon the main question of rotation as an issue of fact, as rested on his previous allegations relating to the New Brunswick and Providence tornadoes: for he appears now to rely chiefly on a *petite guerre* of criticisms, which have little, if any, relation to definite observations; the only evidence on which the question really depends.

I might justly complain of that apparent want of candour which has prevented Dr. Hare from correcting, in any manner, the several mistakes and errors, whether of fact, quotation, or induction, which were pointed out, long since, in my reply to his first series of "objections" and "strictures." It is this want of candour in the discussion that seems to demand these defensive notices and remarks, which perhaps are more necessary from the fact that few persons, probably, engage in a careful and strict analysis and comparison of the observations which have been made in storms.

Dr. Hare now says he had "endeavoured to point out various errors and inconsistencies in the theory of storms proposed by me, or in the reasoning and assumed scientific principles on which that theory had been advanced." But it has never been my purpose to "propose" or "advance" a "theory of storms" founded on "reasoning and assumed scientific principles." This has, indeed, been attempted by others; with what success is best known to attentive inquirers: whereas I have mainly endeavoured to exhibit a matter-of-fact view of the actual phænomena of storms, so far as relates to their progress, the violent rotative winds which they exhibit, and the effect of these winds on the barometer.

Referring to a supposed approval of my views by men of science, Dr. H. says [§ 58], "It strikes me, however, that a fault now prevails which is the opposite of that which Bacon has been applauded for correcting. Instead of the extreme of entertaining plausible theories having no adequate foundation in observation or experience, some men of science of the present time are prone to lend a favourable ear to any hypothesis, however absurd in itself, provided it be *associated with observations*." As already stated, it is "observations" and their results which I have chiefly endeavoured to promulgate. But if it has been attempted to associate a favoured "hypo-

thesis," whether "absurd" or otherwise, with observations on storms, I apprehend it has been by my opponents, notwithstanding that the seeming dislike to observations may appear unfavourable to this conclusion.

In the same paragraph are alleged no less than three quotations in forms of words and connexion such as I did not use; and at least three following paragraphs of the "additional objections" appear devoted to the unamiable attempt to render me obnoxious to distinguished men, which perhaps may render proper the following statement and explanation.

I had incidentally remarked, on the occasion of Mr. Espy's first attempt to discredit certain facts and results which I had stated *, that "the grand error into which the whole school of meteorologists appear to have fallen, consists in ascribing to heat and rarefaction the origin and support of the great atmospheric currents which are found to prevail over a great portion of the globe." And, in allusion to the views found in Sir John F. W. Herschel's treatise on Astronomy, I also said, "Sir John, however, has erred, like his predecessors, in ascribing mainly, if not primarily, to heat and rarefaction those results which should have been ascribed solely to *mechanical gravitation*, as connected with the rotative and orbitual motion of the earth's surface, the influence of which he but partially recognizes in connection with this and another subject of inquiry." By the ill-chosen phrase "whole school," was simply meant, all meteorologists to whose writings I had obtained access. It was an inadvertent form of expression, not particularly noticed by me till after publication, and has probably given more pain to myself than to any one else. I have reason to believe that Sir John Herschel has not thought himself *accused* or *denounced* in these passing and somewhat hurried remarks.

Even if Dr. H. could have succeeded by this *ruse* in covering his apparent discomfiture on the main question of rotation, was it required for the elucidation of science, or consistent with the rules of candour and courtesy, that he should persist in repeated efforts to excite an odium in the minds of his readers?

I had pointed out Dr. Hare's error in alleging that I reject the influence of heat on winds. In now repeating this allegation [§ 63], he complacently intimates, that "It is very possible that his opinions may have changed since he read my '*objections*,' but that he DID REJECT THE INFLUENCE OF HEAT †

* Silliman's Journal, vol. xxviii. p. 316.

† In cases of quotation, where it is proper to notice the bearing of particular words or phrases, I adduce these in small capitals, as above.

when the preceding and following opinions were published must be quite evident." And he then quotes, somewhat inaccurately, part of the subjoined extract as sustaining this allegation; the correctness, or pertinacious unfairness of which, I shall leave unprejudiced readers to determine from the very evidence to which he refers. I had said in immediate connection with the foregoing, as follows:—

..... "But, to prevent being misunderstood, I freely ADMIT that HEAT IS OFTEN AN EXCITING as well as modifying CAUSE of *local* WINDS, and other phænomena, and that it HAS an incidental or subordinate ACTION (though not such as is usually assigned) in the organization and DEVELOPMENT OF STORMS, and that, in certain circumstances, IT INFLUENCES the inter positions of the moving strata of the atmosphere. Its greatest DIRECT INFLUENCE is probably EXHIBITED in what are called LAND AND SEA BREEZES, or in the DIURNAL MODIFICATIONS which are EXHIBITED by regular and GENERAL WINDS. But, so far from being the great prime mover of the atmospheric currents, either in producing a supposed primary north and south current, or in any other manner, I entertain no doubt, that if it were 'possible to preserve [this is the part Dr. H. quotes] the atmosphere at a uniform temperature over the whole surface of the globe, the general winds could not be less brisk, but would become more constant and uniform than ever.'"—*Silliman's Journal*, 1835, vol. xxviii. p. 317.—And with all this before him, he now reasserts that I then rejected THE INFLUENCE OF HEAT!

It appears to dissatisfy Dr. Hare that I should have deemed the first inquiry to be *what are storms?* and not *how are storms produced?* He asks, "suppose that before ascertaining *how* fire is produced, chemists had waited for an answer to the question *what is fire?* how much had science been retarded?" But, waiving any want of analogy between fire and storms, suppose that in treating of fire one chemist should ascribe it to the heat of combustion, another to the smoke and aqueous vapour evolved, while a third should view it as being caused by electricity; would not the proper inquiry then be, what is fire, and what are its obvious phænomena? It appears evident that the laws and phænomena of storms must be first ascertained and established, ere we can advantageously investigate their origin or primary causes.

If Dr. Hare chooses to consider this an endless controversy which he has waged, and that to follow its misunderstandings would be an Ixion task, ought he not to reflect, that grace to acknowledge those "misunderstandings" which it had brought to light would doubtless have shortened its duration?

Paragraphs 66 to 70 Dr. Hare has devoted to some superfluous suggestions found in my earliest paper, 1831, which were virtually withdrawn more than three years since*. He has also joined [§ 68] a passage from that paper with another from a subsequent one, and quotes both as from the latter. The "unresisted rotation" here refers to the seeming non-resistance of the air to a body turning on its own axis; and the *rotative* velocity of a moving body was correctly viewed as being sometimes "accelerated" by the oblique "resistances" of other bodies.

In § 70–73 my opponent labours to convict me of inconsistencies in passages culled from my reply to Mr. Espy; as if any inconsistencies of mine could disprove the rotative character of storms. The alleged inconsistencies result from his confounding cases which I view as distinct, and from some inaccuracies in my choice of terms. The like purpose is evinced in § 74–78, with a collection of passages on the barometer, where Dr. H. seems to confound the space "around the exterior border" of the gale with its "first portion" and "last portion."

In his criticisms on my statements of the changes of wind in storms [§ 79–85], Dr. Hare fails to appreciate the proper distinction between "*suddenly*" and *immediately*, in passages which in their original state and connection are perhaps sufficiently correct; and he would make the statement of an *exception* which "sometimes happens," to be a contradiction or neutralization of the "evidence," or general result. Had he observed carefully he might have found that his fancied analogy derived from the rotary action of a solid is entirely inapplicable to the case of natural eddies and whirls, which are produced by a gravitating force acting from the exterior. He might thus have learned that his hypothetical statement of the law of rotation in fluids does not, at least in all cases, agree with fact, and can in no way alter or affect the vorticular or other rotative action exhibited in nature. Nor can he disprove or annul the fact, that an immediate or a sudden change takes place at the *inner margin* of the violent part of a regular and extensive whirlwind storm, at the border of the central lull or remission of the gale.

His implied allegation [§ 86] that "there is no evidence" that the wind was more violent on the *south-eastern*† side of

* See note prefixed to my article on Hurricanes in the Nautical Magazine for January 1839, and Silliman's Journal, vol. xxxv. p. 201–202.

† This I believe to be Dr. Hare's meaning; for the word "south-western," which he here uses, I deem to be a misprint; else Dr. H. fails to understand himself in this passage; for there is nothing in my views, as set

the gale of August 17th, 1830, than on its north-western side, is opposed by the testimony of Captain Waterman of the Illinois and by the log-book of the ship, as compared with observations made at the same time on the opposite or north-western side of the gale. It was on or near the central line or axis-path of this storm, that only south-easterly and north-westerly winds were successively exhibited; a fact which appears quite sufficient to settle the main question between me and my opponents.

Dr. Hare infers that "in no case would the inner portion of the south-eastern and more violent limb" of a gale or hurricane "be beyond the cognizance of our merchants and insurers;" and then says, that "experience shows that every north-easter brings in a crowd of vessels having only to complain of the violence, not the direction of the wind" [§ 87]. But do the alleged "crowd of vessels" come from far in the *south-eastern offing*? The storm of August 17th, 1830, was at New York a strong "*north-easter*;" and would the Illinois, in the Gulf Stream off Nantucket, have found no cause to complain of the "direction of the wind," if bound to New York or Philadelphia?—this ship having had the wind set in at "*south*," and veering "first to *south-west*, then to *west* and *north-west*," a "perfect hurricane!" "*Experience*" has shown, in a multitude of cases, that in these violent gales, while blowing north-easterly on our shores, the wind is found more easterly, southerly, and south-westerly, in proportion to the increased distance from the coast. This produces a dangerous cross sea; and "our merchants and insurers" *have*, unfortunately, been too often cognizant of the destructive effects.

In [§ 88–91], Dr. Hare has succeeded in showing that a summary passage on the phases of hurricanes in the West Indies, from which he adduces an extract, is not reconcileable with all the local changes in such storms, considered as moving whirlwinds. There are two ways, however, by which this labour might have been lessened or avoided: first, by quoting the next sentence, which suggests qualifications, and second, by referring to the same number of Silliman's Journal [vol. xxv. p. 114–121], where the phases of these gales in the western Atlantic are particularly set forth, together with a key for suiting these explanations to the storms while in West Indian seas, viz. that in the latter region, the direction of the wind, in the corresponding sides and phases of the storms, is

forth, or in the nature of the case, that requires the wind to be stronger on the "south-western" *side* of a storm than on the "south-eastern" side, but rather the contrary.

found "about ten or twelve points of the compass MORE TO THE LEFT [on the compass card], than on the coast of the United States in the latitude of New York."

In the next place, Dr. H. endeavours to show [§ 92-94] that I seem to suppose whirlwinds as capable of being "self-induced." In justice to his readers, however, he should have quoted the entire paragraph from which he has cited my remark, "that whirlwinds and spouts appear to commence gradually and to acquire their full activity without the aid of any foreign causes" (Silliman's Journal, vol. xxxiii. p. 61). But can Dr. Hare prove to us "the aid of any foreign causes?" It is proper to note here, that by the above remark I did not intend to exclude the influence of atmospheric pressure and elasticity, nor variations of temperature and density in and about the body in which gyration is induced. Neither do I disconnect or "isolate" the spirally ascending central motion from the great body of the tornado or whirlwinds as he attempts to do for me.

Dr. Hare finally declares [§ 95], "I do not deem it expedient to enter upon any discussion as to the competency of the evidence by which the gyration of storms has been considered as proved." The friends of science may well be surprised at this. For, if Dr. H. did not intend to discuss the "*evidence*" of gyration, for what useful purpose did he "enter the lists?" or why did he attempt to show facts in disproof? Was it more important to array a series of criticisms and speculations than to bring the question to the test of strict observation and induction? And will not this evasion be received as proof of the weakness of his cause? He says that the competency of the evidence has by Mr. Espy been "ably contested." But has it been so "contested" by that writer, as to be decided adversely in the mind of any strict and careful inquirer, or with such scrutiny and arrangement of the facts alleged as would allow them to speak in their own true language*? Even if Dr. H. should admit gyration to be "sufficiently proved," and "should consider it as an effect of a conflux to SUPPLY an upward current at the axis," would not this imply a *self-elevating power* in this "upward current?" And would not the admission of gyration decide the question in my favour?

But he adds further: "Yet the survey of the New Brunswick tornado, made on *terra firma* with the aid of a compass, by an observer so skilful and unbiassed as Professor Bache, ought to outweigh maritime observations, made in many cases under circumstances of difficulty and danger." Now let me

* Perhaps a partial exception ought to be acknowledged here as relates to one case. See Journal of the Franklin Institute (Philadelphia) for June 1839, p. 372-374.

ask, Is gyration disproved by this survey? I trow not; and apprehend that I have sufficiently shown its results to have been accordant with a general rotative action *.

Still unwilling to admit rotation, he refers to the storm of December 21, 1836, in the terms which follow.

"In like manner great credit should be given to the observations collected by Professor Loomis respecting a remarkable inland storm of December 1836. This storm commenced blowing between south and east to the westward of the Mississippi, and travelled from west or north-west to east or south-east, at a rate of between thirty and forty miles per hour [?]. There appears to have been within the sphere of its violence an area, throughout which the barometric column stood at a minimum, and towards which the wind blew *violently* on the one side only from between east and south, and on the other only between north and west [?]. This area extended from south-west to north-east more than two thousand miles. Its great length in proportion to its breadth seems irreconcilable with its having formed the axis of a whirlwind [!]. The course of this storm, as above stated, was at right angles to that attributed by Redfield to storms of this kind [!]. (Trans. Am. Phil. Soc. vol. vii.)"

We have it here asserted that "this storm" . . . "travelled from west or north-west to east or south-east:" and that "the course of this storm, as above stated, was at right angles to that attributed by" me to other storms; while at the same time we are told that the area, "throughout which the barometric column stood at a minimum," . . . "extended from south-west to north-east more than two thousand miles." Now, in all storms which I have noticed in this part of America, the course and progress of the barometric minimum appears coincident with that of the body or axis of the storm; and as the length of the track thus passed over is quite a distinct thing from the *length* of the storm itself, or from the "area" of the barometric minimum *at any given moment of time*, it appears to follow from Dr. Hare's own statement, that the course of the proper body or axis of the gale was *north-easterly*; *coinciding with the course of other storms*. Moreover, I have not yet seen any evidence which shows that even *one* storm of magnitude in the United States has proceeded in a *south-easterly* course; although such a conclusion has been suddenly adopted, ere now †, apparently with the hope of escaping from a difficulty in which some favourite hypothesis had become involved.

* Article on the New Brunswick tornado, in this Journal, January 1841, p. 20–29.

† Not, however, by Prof. Loomis.

I am aware that Professor Loomis alleges, in his elaborate account of this storm and its attendant phænomena, which I greatly value, although dissenting from some of his conclusions, that "in this case there was no whirlwind." I will only remark, that to me the characteristics of this storm appear to be those of a diffused overland gale of the whirlwind character; the only observations obtained being evidently on the right-hand of the path of its axis. I understand, also, that other inquirers have been led by the evidence to the same result.

The manner in which Dr. Hare has described this storm, and his erroneous allegation in regard to its course, show very strongly the importance of the inquiry, *What are storms?* For, was it the area of the minimum depression of the barometer —or the area of violent winds—or the area of the rain—or the area passed over by the wave of barometric oscillation—or the area of extraordinary changes of temperature—which constituted the proper limits or identity of this storm*?

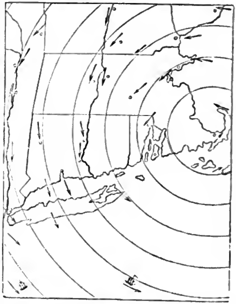
Those readers who may desire to ascertain the general course of the wind in the body of a great storm, without resorting to a process of induction from characteristic facts on one hand, or to the aid of ingenious hypotheses which regard certain alleged but unknown movements of the air in connection with the higher atmosphere on the other, are referred to a schedule and map of observations made at about forty separate localities, at the hour of noon, in the storm of December 1839, which are found in this Journal for January 1841. These observations are believed to exceed in number and accuracy any that have yet been obtained in equal limits, and they are arranged on the map so as to speak their own proper language as simultaneous observations†. Hence they appear to show conclusively, that the violent easterly winds in this American storm were resolved, through a circuitous geographical course, into the strong north-westerly winds which immediately followed the easterly part of the gale; instead of mounting to unknown regions, before opposing winds, as has been alleged by others.

* So far as definitions only are concerned, and these are important in science, it may be proper to adduce the following from Webster, the lexicographer:—

"STORM, *n.* A violent wind; a tempest. Thus *a storm of wind* is correct language, as the proper sense of the word is rushing, violence. It has primarily no reference to a fall of rain or snow. But as a violent wind is often attended with rain or snow, the word *storm* has come to be used, most improperly, for a fall of rain or snow without wind."

† For more extended remarks relating to these observations, see Silliman's Journal for April 1842, p. 112.

The arrows marked on the small geographical sketch which is here annexed, show the direction of wind at some of the principal points of observation eastward of the Hudson river, near the close of the day, when the body of the storm was further advanced in its north-easterly course. The concentric lines, drawn at intervals of thirty miles, are designed to afford better means of comparison for the several observations.



The observations which I have obtained of this storm, and its remote effects, are far more extensive to the southward and eastward than the limits of the map; showing also that in this portion of the storm, the winds in the early part of the gale were blowing from south-easterly and southerly quarters. It is worthy of remark, that if only those observations which are southward of the parallel of Long Island had been obtained and considered, this storm would appear to show an inequality in its phases and an absence of violent north-easterly winds, similar to what is found in Professor Loomis's account of the storm of December 1836, which he was led to pronounce as no whirlwind.

The observations on the map referred to have a further value, inasmuch as they belong to a case which Mr. Espy has exhibited as one of his inward and upward blowing storms; for they show, on a strict comparison and investigation as to *time* and locality in the storm, that, besides inaccuracies, the *coup d'œil* of the observations delineated in his diagrams is illusory, and gives to consecutive winds, which follow each other over the same localities, or in different parts of the storm's path, the appearance of simultaneous and opposing winds, blowing in opposite courses towards each other.

In the case of tornadoes it is necessary to resort to a process of induction to determine both the relative positions in the tornado of the several fallen bodies at the instant of their

prostration, and the general character of the prostrating force which these may conjointly indicate. The course of induction suggested in my two papers on the tornadoes of Providence and New Brunswick, as taken together, are believed to afford sufficient grounds for a correct determination, when applied to the traces of other tornadoes. It is also satisfactory to find, that in the surveys exhibited in the above cases there are several traces of individual objects moving in the tornado, which fully confirm the accuracy of the more general induction.

As regards Dr. Hare's own views of the electrical origin of storms, some notice has been taken of these in Silliman's Journal for October 1842, p. 261–263. Since the discoveries of Franklin, an electrical origin and character has often been conjecturally ascribed to storms. A want of originality in advancing this hypothesis will not weaken any evidence which shall be adduced in its favour; but until it shall have been satisfactorily supported by observed phænomena, it will probably continue to be rejected by scientific inquirers. And were it possible to show an electrical origin in great storms and tornadoes, it would in no wise alter the known fact that a determinate rotative action has been noticed in these storms.

LX. *Notice of some Experiments on Subterranean Electricity made in Pennance Mine, near Falmouth. By* R. W. Fox, *Esq.**

I HAVE already communicated to the Geological Society of London† some results produced by the electric action of two nearly east and west metalliferous veins which have been partially explored in Pennance mine. I have since made other experiments in the same mine, in which ore-points, consisting of copper and iron pyrites in the two veins, were connected by a pair of copper wires, which in most instances acted on a galvanometer or other apparatus at the surface, an end of each wire having been brought up through a shaft for the purpose; about 50 fathoms of wire were employed, although the ore-points in the different veins were only about 14 to 18 fathoms asunder in a direct line.

A galvanometer of not much sensibility was generally used; the needle, which was 2½ inches long, moved on a pivot, and had a coil of fine wire passed 48 times round it. Another galvanometer, consisting of a suspended astatic needle and 140 coils of wire, was also employed occasionally.

* From the Transactions of the Royal Cornwall Polytechnic Society.

† The communication here alluded to will be found in our report of the proceedings of the Geological Society, pres. vol. p. 457.—Edit.

When the former, which call No. 1, was placed in the circuit, the needle was deflected so as to become stationary at 14° to 15° from zero; and it revolved rapidly round the circle when the circuit was broken and restored a few times, the direction of the electricity being from the south vein to the northern one. The other galvanometer (No. 2) suffered a permanent deflection of about 40° when in the circuit. The interposition of a plate of platinum or zinc at either of the ore-points, or of a *point*, instead of a considerable surface of metal, did not affect the direction or force of the currents; they were, moreover, constant in both these respects during more than eight months that the two veins were connected by the wires, and a part of this time the mine was filled with water in consequence of an accident to the machinery. Ore-points in the two veins situated within two or three feet of the others respectively, were at one time connected by a second pair of copper wires of the same lengths as the first; both sets of parallel wires being kept apart, and insulated from the sides of the levels or galleries by poles stretched across the latter at short intervals.

When galvanometer No. 2 was placed in the second circuit, No. 1 remaining in the other, the needle of the latter receded at least 2°, standing at 12°, instead of 14° or 15°; and the former stood at 5° or 6° less than it did when only one circuit was established. On breaking either of the circuits, the deflection of the needle in the other circuit was increased to its original amount; and when *both* pairs of wires were connected with only *one* of the instruments, the effect was almost precisely the same as that produced by one pair alone,—not greater certainly.

A copper and zinc pair of plates of about 6 inches surface, separated by a piece of cotton cloth moistened with water, was placed in the circuit, and when the currents from this source and the veins *coincided* in direction, the needle of galvanometer No. 1 stood at about 10°, that is, at less than it did when acted upon by the subterranean electricity alone, and when the deflection caused by the latter was afterwards opposed by the action of the plates, the needle went back to zero, and even sometimes passed a little beyond it in the opposite direction. These anomalies may perhaps be referred to the low conducting power of the moistened cotton, which, small as its thickness was, very probably interrupted the transmission of the electricity more than the 14 or 18 fathoms of strata or "*Country.*"

On taking the voltaic elements from the circuit and connecting them with the galvanometer, so as to form a separate

circuit acting in an opposite direction to the electricity from the mine, the deflection showed a difference in favour of the latter, and indeed this was the case when the interposed cloth was moistened by a very weak solution of common salt.

The electro-magnetic and decomposing effects of these subterranean currents also afforded unequivocal evidence of their energy. A helix of copper wire fixed round a small horseshoe-shaped bar of iron, was placed in the circuit formed by the wires from the veins, when the bar became so magnetized as to cause a compass needle $1\frac{1}{2}$ inch long, at the distance of nearly half an inch, to oscillate through an arc of about 70°, when the circuit was alternately made and broken a few times.

A solution of hydriodide of potash was found to have been decomposed after it had been left in the circuit for rather more than a day.

The endosmose action occurred in various experiments, but it may be sufficient to give one example. Sulphate of copper in solution was put into both branches of a U-shaped glass tube with clay in the bent part of it, the surface of the fluid in one branch standing half an inch above that in the other. A piece of silver wire was plunged into each of them, the upper end passing out through sealing wax, with which the extremities of the tube were stopped, and the apparatus was placed upright in the circuit, with the wire in the higher column of the fluid connected with the negative wire. In the course of a few days this column was found to have risen one-eighth of an inch, the other having fallen in an equal degree, showing that the greater pressure of the higher column was superseded by the force of the electric action.

When small cylinders of copper pyrites were substituted for the silver wires in the branches of the bent tube, not only did the endosmose action occur, but the copper ore, forming the negative pole, had its surface gradually changed to vitreous copper in the course of two or three days*, the other ore-pole remaining unaltered. The same change was produced, and apparently with equal facility, when solutions of other salts, as carbonate of soda or common salt, were substituted for that of sulphate of copper in both branches of the tube. The cylinders of copper pyrites used in these experiments were long enough for the upper ends to project above the mouths of the tube, where the opposite wires were attached to them respectively, and these were well coated with sealing-wax dissolved in alcohol, to prevent the access of moisture to any part of the metal, and indeed all but the lower portions of the ore were coated in like manner.

* Some of the ore thus changed was at the last Polytechnic Exhibition.

In some instances the cylinders of copper pyrites were allowed to remain in solutions of sulphate of copper in the bent tube for several weeks, when deposits of oxide of iron were found coating the inside of the tube about the negative pole. These results remind one of the ochrey appearance observed in rocks inclosing much vitreous copper, a fact noticed by my friend Joseph Carne; and it may be worth while to inquire how far the proportion of "*gossan*" in copper veins may be connected with the quantity of vitreous ore contained in them.

Since the foregoing experiments were made, I have obtained an electro-type copper plate $1\frac{1}{2}$ inch long, $1\frac{1}{4}$ wide and $\frac{1}{30}$ of an inch thick, by the agency of these subterranean currents. The apparatus consisted of a porous earthenware vessel, resting on wooden legs in a larger one; both were partly filled with solutions of sulphate of copper, an engraved copper plate attached to the negative wire being placed in the outer vessel, and another plate of copper attached to the positive wire in the inner one. After a few days it was observed that crystals of copper had been formed on the negative plate, but it was nearly two months before the apparatus was removed from the circuit, when the deposited metal was detached from the plate, having received its impression, VI INSITA TERRÆ. Whilst this experiment was in progress at the surface, the water, as I have before mentioned, invaded the mine, but without interrupting the process; it appeared, indeed, that the electric action was rather increased than diminished by this circumstance.

Before the influx of the water, an ore-point in the north vein was connected with *rock* near the south vein (generally the wall of the vein), and an ore-point in the south vein was likewise connected with *rock* near the north vein, in both which cases currents more or less feeble were detected passing towards the latter through the wires, which were insulated, as before, by wooden poles stretched at intervals across the galleries. It is probable that the moisture on the rocks conducted the electricity from the ore to the metal, however imperfectly, and when different metals, as platinum and zinc, were successively substituted for the copper in contact with the rocks, the currents were modified in their force according to the metal employed, but were seldom changed in their direction. The action was most decided when the place of contact with the rock was near ore; and sometimes the end of the wire, or rather the piece of copper attached to it, was rubbed by an assistant against the walls of one of the veins or the sides of a "*cross-cut*" between them. Under these circumstances the astatic needle was several times suddenly much deflected, and the parts of the rocks from which this increased action

proceeded having been marked, they were broken away, when iron pyrites was in every instance found imbedded in them; and there can be no doubt that the smallest branch of copper or lead ore might have been detected in like manner.

On several occasions the ends of the opposite wires were placed in contact with the rocks near the two veins, when there still appeared to be a tendency in the currents to pass in the same direction, but often they could not be detected, or were too feeble for their direction to be determined with certainty. Pieces of copper pyrites attached to the wires and imbedded in wood, were likewise used instead of the metal for producing contact with the rocks, and with still less effect; and when the contact was made with platinum and zinc in succession, the currents were in opposite directions, and in accordance with the action of those metals respectively; so that the existence of independent currents under the circumstances described, though more than probable, was not clearly proved. Electricity, generated by a pair of zinc and copper plates, was transmitted through the rocks between the two veins from north to south, and also from south to north, in order to detect any independent currents traversing the rocks by a *differential* effect on the needle. This method appeared likely to be a very delicate test of electric action in rocks, but no decided results were obtained, the currents passing in opposite directions apparently with equal facility, at least the few experiments hitherto made in this way have not led to any satisfactory conclusions relative to the point in question. It should be remarked, however, that the astatic needle employed was inconveniently sensitive, and was often set in motion when the cause was not very obvious. With needle No. 1 the case was widely different, as it could scarcely be moved by any subterranean currents that were not tolerably energetic, such as were produced when *both* the wires were in contact with *ore-points*, and then, as has been stated, it often revolved rapidly.

It has been long known that electric currents will traverse a very considerable thickness of rock or strata*; but in what degree this property may be modified by the nature or texture of the rocks, the saline contents of the subterranean water, or the proportion of ores included in the circuit, remains to be ascertained. If the influence of these different circum-

* Many instances of this occur in my paper "on the electro-magnetic properties of metalliferous veins," published in 1830, in the Phil. Transactions, p. 399. I have long ago seen a *very feeble* current act on a sensitive galvanometer after it had traversed nearly a quarter of a mile of strata, and stronger currents would probably be detected in like manner after having passed many times that distance under the surface.

stances should greatly vary, electric currents generated by given elements might be rendered available on various occasions;—to ascertain, for instance, the connection of saline springs not very distant from each other, often appearing at the surface or in mines; or of a metalliferous vein discovered in one place, with a vein which has been worked for ore in another. The conducting power of the circuit at Pennance mine, already described, was in this way found to equal that of a tolerably strong solution of common salt, the current in the latter experiment having to traverse an inch of the solution and short copper wires to complete the circuit. The conducting power of the rocks or strata in this case, therefore, appeared to be very great.

When some sulphate of copper was added to the solution, the conducting power of the latter exceeded that of the strata. Glass tubes filled with solution of salts in different known proportions might be used as tests in experiments on the relative conducting power of different strata, and they might be referred to as standards in describing the results.

LXI. *On the Constitution of the Subsalts of Copper.*—No. I. *On the Subsulphates. By* J. Denham Smith, *Esq.**

THE results of several analyses of some of the basic salts of copper made at a former period not agreeing with the constitution ascribed in many instances to these compounds, again directed my attention to their composition, and further experience has confirmed this disagreement, showing either that the analytic results are in one case incorrect, or that the composition of these salts, prepared at various times by the same method, is not constant.

The mode adopted for determining the composition of the subsulphates of copper, was to dissolve one portion of the salt in pure hydrochloric acid, and ascertain the quantity of sulphuric acid by a salt of barytes. To estimate the proportion of black oxide of copper, the course pursued in the experiments alluded to, was solution of another portion of the salt, under examination, in dilute sulphuric acid and precipitation by a caustic alkali from the boiling solution, carefully washing, igniting, and weighing the precipitate. This mode, however, is open to the objection of the possible adherence of portions of the precipitant, or other foreign matter, to the oxide; and as I subsequently found that this class of salts when exposed to a lengthened and bright ignition, care being taken not to fuse the oxide, loses the whole of its sulphuric acid, I adopted

* Communicated by the Chemical Society; having been read April 4, 1843.

this latter mode of estimating the oxide, it being one which appears to me free from objection : I would remark that the residual oxide after this ignition was always examined for sulphuric acid, and if any was detected the experiment was rejected. The deficiency of weight between that of the salt operated on and the sum of the sulphuric acid and oxide of copper, was estimated as water.

This method of estimating the water may be objected to as an indirect one, but I consider it more likely to be correct, where a substance possesses so simple a composition as in the case of these subsulphates, than any mode would be that could be devised of actually obtaining and weighing the water; especially as these salts were thoroughly washed until no soluble matter could be detected in the washings, and dried either in a water-bath or on a porous stone, and exposure to the atmosphere at the temperature of the laboratory, 50° to 80° Fahr.

In those instances where the results of my analyses were not in accordance with those cited by Berzelius, Graham, Kane, Thomson and others, the examination was at times repeated, sometimes thrice, and the mean of these analyses taken in estimating the composition of the salt. The modes of preparation and the constitution of these salts I shall classify in the order of their composition, and at the same time notice discrepancies, when such occur, between my results and those of the analysts who have preceded me.

Trisulphate of Copper.—By boiling an equivalent of oxide of zinc and two equivalents of sulphate of copper together, the subsulphate noticed by Berthollet, of a bright green colour with a shade of blue, is obtained. The mean of two analyses gave, from 50 grs. of this salt, 33·97 grs. of sulphate barytes and 34·3 grs. of oxide of copper. The composition deduced from these results would seem to indicate the formula $2SO_3\ 6CuO,\ 3HO$; but I am inclined to consider this salt as really consisting of $SO_3\ 3CuO,\ 2HO$, a constitution almost exactly borne out by the second of these analyses, agreeing more nearly with Brunner's analysis of the subsulphate obtained in this way, and also with the composition of some subsulphates prepared in a different manner, but which indicate the composition $SO_3\ 3CuO,\ 2HO$, which will give—

	Theory.	Experiment.
Sulphuric acid . . .	11·24	11·65
Oxide of copper . . .	33·72	34·30
Water	5·04	4·05
	50·00	50·00

By precipitating 250 grs. of crystallized sulphate of copper

with excess of potash, washing the brownish-black precipitate with hot water until free from alkali, and boiling this with 250 grs. of sulphate of copper, a light green-coloured powder was produced, the boiling and digestion were continued for forty-eight hours, and the liquor was then evaporated to dryness. The mass was treated with water to dissolve out uncombined sulphate, and washed until free from soluble matter; this when dried gave a pale green powder weighing 172 grs.: of this—

36·32 grs. gave 23·8 grs. of sulphate barytes = 7·35 of sulphuric acid in 32·52 grs., which quantities afforded by ignition 21·56 grs. of oxide of copper; this indicates the formula SO_3 3CuO, 2HO, or 32·52 grs. consist of

	Theory.	Experiment.
Sulphuric acid . . .	7·31	7·35
Oxide of copper . . .	21·94	21·56
Water	3·29	3·61
	32·52	32·52

This salt is also obtained when less than an equivalent of oxide of copper is boiled with an equivalent of sulphate of copper.

In Dr. Kane's tabular view of the sulphates of copper (Transactions of the Royal Irish Academy, &c., vol. xix.), this salt, the trisulphate of copper, is not noticed; but Berzelius has described one, assigning to it 3 equivalents of water. If such a salt exists I have been unable to obtain it; and although I admit that such a composition is by no means improbable, seeing that another subsulphate exists in which the number of equivalents of water and oxide of copper are equal, I am inclined to consider that the third equivalent of water in Berzelius's salt was hygrometric, as in no one instance, although this salt was prepared at several distinct periods and in the mode described by Berzelius, did I obtain results indicating an approximation to the constitution SO_3 CuO, 3HO.

The composition I have assigned to this salt agreed with that quoted by Dr. Thomson, as arrived at by Brunner from the analysis of the subsulphate prepared by Berthollet's process.

Tetrasulphate of Copper.—This salt is obtainable in a great variety of ways. It is precipitated when a cold solution of sulphate of copper is mixed with an insufficient quantity of carbonate or of caustic soda, or potash to completely decompose it. It may be prepared by digesting together cold,—equivalents of sulphate of copper, and of well-washed precipitated oxide of copper; by adding a solution of potash to a warm solution of sulphate of copper until a greenish-blue precipi-

tate falls, and no copper remains in solution; by treating a cold solution in the same manner; by acting on the ammoniacal sulphate by a large quantity of water, &c. Numerous analyses of this salt, prepared by the various processes above mentioned, have only served to confirm the correctness of the formula, SO_3 4CuO, 4HO, assigned to it by Professor Graham and Dr. Kane; it is therefore useless to quote any of these results.

Dr. Kane, in the paper before mentioned, states that "this salt when heated does not lose water until the temperature rises above 300° Fahr., but then loses all." I find, however, by exposing this salt to a temperature of 400°—470° Fahr., that it assumes a grass-green colour accompanied with the evolution of water.

When 43·2 grs. of the dingy greenish-blue powder, obtained by digesting an equivalent of precipitated oxide of copper with an equivalent of sulphate in the cold, were exposed to the above temperature, it changed to a decided green colour and lost 1·63 grs., this is equal to 8·9 grs. of water from 236, the equivalent number of the tetrasulphate of copper, which indicates SO_3 4CuO, 3HO as the constitution of this green subsulphate.

27·07 grs. of the blue subsulphate became of a grass-green colour and lost 1·04 gr. of water, equivalent to 9·07 grs. from 236 grs., and indicating the formula SO_3 4CuO, 3HO as the constitution of this green subsulphate, arising from the loss of an equivalent of water by the blue subsulphate, SO_3 4CuO, 4HO. This green salt on analysis gave from 13·43 grs. 9·4 grs. of black oxide of copper, and 6·05 grs. of sulphate barytes from 11·62 grs., proving its composition to be as above stated, SO_3 4CuO, 3HO, or,

	Theory.	Experiment.
Sulphuric acid . . .	2·37	2·4
Oxide of copper . . .	9·47	9·4
Water	1·59	1·63
	13·43	13·43

This salt is similar to that analysed by Brunner, obtained by boiling equivalents of sulphate of copper and sulphate of potash together, until after repeated washings and boiling no sulphuric acid could be detected in the solution.

When this green salt is moistened, or even boiled with water, it does not change colour, nor re-combine with the equivalent of water it had lost, as I had anticipated from the statement of Dr. Kane, that "at a temperature above 300° it loses all its water, and the brown powder, if exposed to the air, re-absorbs water slowly; if moistened it combines with

the water, rapidly evolving heat, and regains its original proportion, and also its proper colour."

There exists another hydrate of the tetrasulphate, obtained by precipitating a very dilute solution of sulphate of copper by a solution of potash, also much diluted, adding the alkali until the supernatant fluid restored reddened litmus to blue; this when dried was an extremely light powder of a very pale blue colour, altogether differing in appearance to the other tetrasulphates; 18·63 grs. of this salt gave 8·28 grs. of sulphate of barytes, and 29·38 grs. afforded 19·33 grs. of black oxide of copper, indicating the formula SO_3 4CuO, 5HO, or

	Theory.	Experiment.
Sulphuric acid . . .	4·79	4·51
Oxide of copper . .	19·19	19·33
Water	5·40	5·54
	29·38	29·38

Pentasulphate of Copper.—This salt is obtained when potash is added to a solution of sulphate of copper, until the alkali is slightly in excess and a light blue-coloured precipitate is obtained; this, unless rapidly washed, becomes gradually dingy, and finally turns to a dark greenish-black colour, probably owing to the loss of combined water. This change often takes place, entirely or partially, during the drying of the precipitate, even when dried by exposure to air, without artificial heat; when obtained free from this blackening effect it is a light powder of a blue colour, a more decided tint than that of the tetrasulphate with five equivalents of water; upon analysis, which was frequently repeated, the constitution of this salt was found to be SO_3 5CuO, 6HO. The results of two analyses of this salt, prepared at different times, are subjoined: 32·2 grs. of the salt gave 21·6 grs. of oxide of copper, and 22·36 grs. gave 9·74 grs. of sulphate barytes, or

	Theory.	Experiment.
Sulphuric acid . . .	4·38	4·83
Oxide of copper . .	21·90	21·60
Water	5·92	5·77
	32·20	32·20

24·3 grs. gave 16·47 oxide copper, and 17 grs. gave 6·52 grs. sulphate of barytes, equal to

	Theory.	Experiment.
Sulphuric acid . . .	3·31	3·2
Oxide of copper . .	16·53	16·47
Water	4·46	4·63
	24·30	24·30

32·2 grs. heated on a sand-bath until it assumed an olive-

green tint lost 2· grs., and 36·13 lost 2·2 grs. of water, equivalent to 2 equivalents of water from 294, the equivalent number of the pentasulphate of copper, SO_3 5CuO, 6HO, thus altering its constitution to the formula SO_3 5CuO, 4HO. Besides the subsulphates of copper already described, two others are stated to exist, a disulphate and an octosulphate of copper. Dr. Thomson describes the first, the disulphate, as produced " when crystals of the blue sulphate are dissolved in water and the solution boiled for a long time with a quantity of black oxide of copper, equal to that contained in the salt," and states it to consist of SO_3 2CuO, but gives no water as a constituent of it. L. Gmelin, on Thomson's authority, directs that equal equivalents of the sulphate and oxide of this metal be digested together for some months to obtain the green disulphate. I tried both these plans; by boiling, at various intervals during ten weeks, equivalents of sulphate and oxide, I obtained the trisulphate of copper, SO_3 3CuO, 2HO, a green-coloured powder; by digesting an equivalent of each for thirteen weeks the tetrasulphate, SO_3 4CuO, 4HO, was produced; even when excess of sulphate of copper was boiled with the precipitated and washed oxide still the subsulphate, SO_3 3CuO, 2HO, was formed; and when in addition to these unsuccessful attempts to obtain it, the modes described being so distinct and easy of execution, we take into consideration that water is not mentioned as a constituent of this disulphate, which is also described as a green-coloured powder,—and no salt of copper whatever is known that possesses a green or a blue colour unless water be present—I am compelled to deny the existence of Thomson's disulphate. Of the non-existence of the octosulphate of Dr. Kane I am not prepared to speak so decidedly; the evidence of the existence of such a salt is so complete and circumstantial, that on a *primâ facie* view of the description and analysis of this salt in the paper " On the Compounds of Ammonia," it almost compels belief. The production of this very singular salt is dependent, according to Dr. Kane's description, upon " the quantity of alkali employed in the precipitation; where potash had been used, there were two distinct precipitates produced, the one the bluish-green generally described, the other ' the octosulphate' of a clear grass-green, resembling that of hydrated oxide of nickel. When ammonia was employed the former alone was produced, and the formation of the latter was found to occur where the whole of the copper had been thrown down, but the liquor had not yet begun to react alkaline...... It was found in the first instance accidentally, but I have since seldom failed in preparing it completely pure." The process

thus laid down I have followed; and have tortured it in every way, using hot and cold, weak and strong, solutions—adding potash till the solution was perfectly neutral, and also until it became distinctly alkaline, but all to no purpose; I could in no way, nor in a single instance, obtain a salt containing less sulphuric acid than the pentasulphate, SO_3 5CuO, 6HO. When an excess of alkali was added, the precipitate would often change to a greenish-brown tint, but all my efforts to obtain the green salt described by Dr. Kane were fruitless. The composition assigned to this salt, SO_3 8CuO, 12HO, is a most singular and extraordinary one, and on account of this singularity it deserves considerable attention; if a portion of this salt be sent to me I will submit it to analysis, and should it really be found to exist, shall be happy to bear witness to that effect; but at present must confess that I do not believe in its existence. I perhaps may be pardoned the suggestion, but it is not impossible, if the solution of potash was not quite free from carbonic acid, that a mixture of subsulphate, hydrate, and carbonate of copper might be obtained, which on analysis, reckoning the carbonic acid expelled by the second heating with spirit lamp as water, would afford results closely approximating to such a constitution as SO_3 8CuO, 12HO.

On a review of these subsulphates, the question respecting the function of the water contained in them naturally presents itself, and it is one well worthy consideration on account of the highly distinguished authorities who have advocated particular theories on this subject, which at present are usually admitted, or at least are not disputed.

Professor Graham, in the paper "On the Constitution of the Oxalates," &c. &c., seems to decline the question, for speaking of the subsulphates of copper and zinc, he says, "When most successfully prepared they were found to contain four atoms of metallic oxide to one of acid (instead of three atoms of oxide, as M. Berzelius supposed)." Berzelius supposed rightly, there is a trisulphate, "together with four atoms of water. I have not hitherto been able to form a distinct idea of their constitution, or to decide between different views which may be taken of it;" yet in the previous page Mr. Graham writes, "In a former paper upon water as a constituent of sulphates, I examined particularly the constitution of hydrated sulphuric acid and of the sulphates of the magnesian class of oxides" (copper is included in this class). "All these salts contain one atom of constitutional water;" and again, "all salts are neutral in composition." Now it appears to me that if these two laws be true, that not only should all these subsulphates of copper lose all their com-

bined water save one equivalent, but also, as all salts are neutral salts, that water replaces sulphuric acid with the oxide of copper, thus playing the part of an acid, or *vice versâ*; the first view is not borne out by experiment, and the second exhibits either water or oxide of copper in a new and singular point of view, as possessing both basic and acid characters; in sulphate of water or sulphate of copper, a base; in the subsulphates of copper, either the water or the oxide of copper, an acid. Whether the true meaning has been attached to the laws quoted as laid down by Mr. Graham, I am unable to say. If I have misunderstood his meaning, the misstatement has been unintentional, and is owing to having accepted these sentences for what they express, viz. that all sulphates of the so-called magnesian class of oxides contain an atom of constitutional water, and that all salts are neutral. I presume, however, that the subsulphates of copper will be added to the already somewhat long list of specified exceptions to this latter law. In his 'Elements of Chemistry,' p. 169, Mr. Graham, speaking of subsalts, says, "The compounds of the present class appear to be salts which have assumed a fixed metallic oxide in place of this water," that of crystallization, "they may therefore be truly neutral in composition, the excess of oxide not standing in relation of base to the acid." And this passage surely bears out the meaning I have attached to the former expressions, and is wholly incompatible with the observed facts relative to the subsulphates of copper; for under this view there should be but one, viz. $SO_3\,HO + 5CuO$, corresponding with the crystallized blue sulphate of copper, a compound at present not known to exist.

I now come to Dr. Kane's views on this point. In the paper already referred to, he likewise assumes as a "general principle that the transition from the neutral to the basic condition in salts takes place by the replacement of water by metallic oxide, has, as I conceive, received the fullest confirmation." This is clear and distinct, but I submit that this "general principle" is overthrown by what we have seen to be the constitution of the various subsulphates of copper. Further on it is stated, that "a great number of circumstances conspire to render the derivation of the basic sulphates of the magnesian class, from the neutral condition, exceedingly complicated. Thus the neutral salts crystallize with quantities of water variable within very extensive limits, and the proportion of metallic oxide by which it may be replaced is subject to variations equally wide: moreover, the replacement of the water by metallic oxide may be but partial, and hence the

different hydrated conditions in which the basic salts exist. From these causes may be deduced the possible existence of a very extensive series of basic sulphates varying considerably in type, and *subject only to the one restriction*, that in all their different conditions the sum of the equivalents of water and metallic oxide shall always be equal to the sum of the same constituents in some one of the forms in which the neutral salt may crystallize." To show how far the "one restriction" of this law holds good with the subsulphates of copper, we find that the blue and the green crystallized neutral sulphates of copper contain respectively 1 equivalent base and 5 water = 6 equivalents of water and metallic oxide to one of acid, and 1 equivalent of oxide and 1 equivalent of water equal to 2 equivalents to one of acid, whilst the subsulphates contain respectively five, seven, eight, nine, and eleven equivalents of oxide and water together, combined with one of acid; thus in no one instance do the subsulphates of copper agree with the law laid down by Dr. Kane for regulating the constitution of basic sulphates. True it is that Berzelius assigns the composition SO_3 3CuO, 3HO equal to 6 equivalents of water, and metallic oxide to one of acid; but, as has been before stated, I believe one of these equivalents of water to be hygrometric, and that its true constitution is SO_3 3 CuO, 2 HO.

Being thus compelled to differ from those distinguished chemists who have preceded me in these inquiries into the constitution of subsalts and of the subsulphates of copper, I would submit the following idea of the constitution of this class of salts, at the same time distinctly refusing to draw any general conclusion from a rule which I only know is in accordance with observations upon one particular class of salts. I consider the subsulphates of copper to exist as anhydrous sulphate of copper combined with two or more equivalents of hydrated oxide of copper; these compounds, in most instances, unite with definite proportions of water, precisely in the same manner as some neutral and acid salts combine with water of crystallization, which like them they part with at stated elevations of temperature.

This view is completely borne out by the subjoined tabular arrangement of all the subsulphates of copper I have been able to procure. Could I consent to consider either the water or the oxide of copper as standing in the same relation to the other as an acid does to its base, Mr. Graham's theory of the constant neutrality of salts, as applying in this instance, might readily be admitted; but believing both of them to be only capable of acting as basic oxides, I am compelled to reject it, and admit the existence of basic as I do of acid salts.

Sulphates of Copper.

Anhydrous neutral sulphate.	SO_3 CuO.	
Green neutral sulphate .	SO_3 CuO + HO.	(Thomson.)
Blue neutral sulphate .	SO_3 CuO + 5 HO.	
Trisulphate	SO_3 CnO + 2 CuO 2 HO.	
1st tetrasulphate . . .	SO_3 CuO + 3 CuO 3 HO.	
2nd tetrasulphate . . .	SO_3 CuO + 3 CuO 3 HO + HO.	
3rd tetrasulphate . . .	SO_3 CuO + 3 CuO 3 HO + 2 HO.	
1st pentasulphate . . .	SO_3 CuO + 4 CuO 4 HO.	
2nd pentasulphate . . .	SO_3 CuO + 4 CuO 4 HO + 2 HO.	

LXII. *On the Spontaneous Change of Fats.* *By* W. Beetz, *Esq.**

WE sometimes find in various parts of mines, which have not been worked for a considerable time, fragments of a white brittle substance having frequently the appearance of fat, but at times so changed that it presents more the aspect of a mineral body; I am not aware that any one has examined this substance, and was consequently very much pleased at receiving some pieces of it from different mines.

The first specimen was very brittle, so that it could be rubbed to a fine powder; its appearance was that of tallow. The exterior was a little covered by sesquioxide of iron, but the interior was quite clean. It was found in the "Old Man" iron mine, Xiffan near Runderroth, in the district of Oberberg. It was dissolved by boiling alcohol without residue, but on cooling was deposited as a flocculent precipitate. Warm æther dissolved it very easily, and from this solution it could be crystallized. When boiled with an alkali, it was converted perfectly, but not very easily, into soap. Submitted to destructive distillation the products were the same as those of all fats containing glycerine, as was fully evidenced by the intense and peculiar smell.

It melted at 59° C. into a perfectly clear liquid.

The analyses of this body showed its composition to be as follows:—

I. 0·323 gr. of the substance gave 0·349 gr. of water and 0·904 gr. of carbonic acid.

II. 0·314 gr. gave 0·352 gr. of water and 0·878 gr. of carbonic acid.

III. 0·316 gr. gave 0·361 gr. of water and 0·878 gr. of carbonic acid.

These results, calculated to the hundred parts, give the following results:—

* Communicated by the Chemical Society; having been read April 18, 1843.

	I.	II.	III.
Carbon . .	76·32	76·25	75·78
Hydrogen .	12·00	12·45	12·69
Oxygen . .	11·68	11·30	11·53
	100·00	100·00	100·00

This composition is the same as that of stearine from mutton tallow, according to the authority of Lecanu. I insert here an analysis of stearine, made by Liebig and Pelouze, and the result of their calculation, in order to compare them with the analysis made by myself.

Liebig and Pelouze.	Calculated.
146 C = 76·14	76·21
236 H = 12·30	12·18
17 O = 11·56	11·61
100·00	100·00

The solubility of the substance is also the same as that of stearine, and the difference of the temperatures at which the two bodies melt is very small. The melting point of stearine is 62°, that of the body under examination 59°, while that of tallow is not more than 37° C.

A portion of the body was saponified by soda. The soap was solid and hard, and when dissolved in hot water it formed on cooling a gelatinous mass, even when the quantity of soap was very small. It was decomposed by hydrochloric acid, and the fat acid obtained in this manner was also hard and brittle; its melting point was 60° C.

On burning the acid with oxide of copper, the following result was obtained:—

0·3215 gr. of the substance gave 0·3715 gr. of water and 0·836 gr. of carbonic acid.

This composition, calculated to the 100 parts and compared with the analysis and the calculation of stearic acid made by Berzelius, gives the following results:—

	Calculated.	Berzelius.	Found.
70 Carbon	= 79·963	80·145	79·40
134 Hydrogen	= 12·574	12·478	12·81
5 Oxygen	= 7·463	7·377	7·79

It cannot be supposed that this fat came into the mine in the form of stearine, but most probably it had been a miner's candle, and had probably been changed into stearine by the continuous action of water, for the composition of stearine differs from that of tallow in its containing a greater per-centage of carbon. Tallow contains to 100 parts of carbon, 14·81 hydrogen and 11·76 oxygen. Stearine to the same quantity of carbon, 16·02 hydrogen and 15·14 oxygen; so that this change can be accounted for by supposing that the tallow

had combined with water, at the same time that it lost carbonic acid. The conditions necessary to this process are always found in mines, water is present, and the admittance of air is not required.

Another piece of fat was found in the mine Frederic near Tarnowitz; its appearance was the same as that of the first substance. By boiling alcohol and æther it was not perfectly dissolved. The substance was carefully cleaned outside, and then the exterior portions as well as the interior examined. From the exterior 0·317 gr. were finely scraped and boiled with alcohol, then filtered and washed. The filtered solution was precipitated by water, and weighed 0·057 gr. The residue was boiled with hydrochloric acid; the weight of the fatty body that was hereby separated was 0·232 gr. The liquid contained nothing but lime in solution, which determined as carbonate weighed 0·044 gr., and is equivalent to 0·025 gr. of caustic lime. The melting point of the fat, soluble in alcohol, was 58° C., that of the fat acid, separated by hydrochloric acid, 60° C. The first fat was from all its reactions stearine, the second stearic acid, so that the substance was composed of 0·257 gr. stearate of lime and 0·057 gr. stearine, or in 100 parts of 17·98 stearine, 7·88 lime, and 73·18 stearic acid. The proportion of lime and stearic acid in the soap is the same as that in the soap prepared in the common manner; for if we calculate the composition of the 81·06 grs. lime-soap, which the substance contains, we obtain the following values:—

	Found.	Calculated.
Stearic acid . .	73·18	73·20
Lime	7·88	7·86
Stearine . . .	17·98	
	99·04	

This composition is confirmed by a second combustion of another portion of the same body.

0·317 gr. of the substance gave 0·833 gr. of carbonic acid and 0·338 gr. of water. If we calculate these numbers to their equivalents in 100 parts, and if the substance consists indeed of stearine and stearate of lime, the resulting values must be the same as those which can be calculated from the known composition of stearine and stearic acid. This is the case in the following table.

	Stearic acid.	Stearine.	Calculated.	Found.
Carbon .	58·54	13·70	72·24	71·66
Hydrogen	9·22	2·19	11·41	11·84
Oxygen .	5·42	2·09	7·51	8·62
CaO . .	...	...	7·88	7·88
	73·18	17·96	99·04	100·00

0·986 gr. of the inner portion of the substance was boiled with æther, the solution evaporated and the fatty matter fused, it weighed 0·709 gr.; the residue, 0·277 gr., was decomposed by hydrochloric acid; it gave 0·249 gr. of a fat acid and 0·047 gr. of carbonate of lime, equivalent to 0·026 gr. of lime. If we calculate these values to the 100 parts and compare them with the numbers derived from the known composition of stearate of lime, and deduct the ascertained amount of the stearate, the remainder will represent the stearine.

	Found.		Calculated.
Lime	2·64	28·09	2·70
Stearic acid . .	25·25		25·39
Stearine . . .	71·90		71·91
	99·79		100·00

In the inner part, consequently, the substance consisted more of stearine and less of soap than on the exterior parts, so that the saponification, which began through the medium of the adjacent lime, is not yet complete, but is more advanced in the exterior portions than the interior.

This substance probably had the same origin as the first specimen; it is very similar to [the] adipocire discovered by Fourcroy, and which Chevreul has proved to be human fat partly saponified, the bases of which are ammonia, from the nitrogenous compounds of the human body, and magnesia and lime, from the bones. The process of saponification took place in this, as well as in the described substances, by the long-continued action of the materials on each other, which action is not as yet perfected. The change of fats into stearine has many analogies in the manufacture of candles. The manufacturer of stearine candles prefers using tallow of from one to two years old, because it yields a larger profit, and the fat looks a little whiter than fresh tallow. If a piece of mutton tallow is broken asunder and lies in a warm room, the fracture-surface is soon covered with an oily substance, the elaine, which is present in great abundance. The surface of an old fat, treated in the same manner, remains dry and without this oily aspect, because it does not contain so much elaine. The fact that tallow candles become whiter after some time, is not of this nature, because it demands the presence of air, and the candles do not become hard, but tough.

Another occurrence that gave rise to a product quite resembling the described fats must here be mentioned. In a manufactory of candles in Berlin the tallow was poured into a large box; the door of this, by which the fat was taken out, was one day not well closed, some of the fat consequently ran out, and remained under the box during ten years. After this

time the box was taken away, and the fat was now found quite hard and brittle. I have not been able as yet to obtain any of this substance in order to examine the changes it had undergone. In a fat of a few years old it would be always difficult to show the change into stearine by an analysis of the elements, for the change could not as yet be very advanced, and the composition of the fat would not therefore be very different from that of stearine. The conditions necessary to the above-mentioned change are the same in the mine as in the cellar where the box stood; in both water was present, but not a free passage of air. The change in the first two cases had been going on for a long time, for it can be shown that the fat had been in the mines during more than a hundred years. If this one condition could be supplied by another, so that the fat could be changed readily by an artificial process into stearine, it would not be necessary to consider the elaine as a disagreeable addition and to connect it to a bad and cheap product, but the whole substance could be worked as stearine, or from the whole quantity stearic acid could be obtained.

The substances described prove the truth of the supposition which Liebig has made in his last treatise "On the production of Fat," that liquid fats can be changed into hard ones.

LXII. *On certain Improvements in the Instrument, invented by the late* Dr. Wollaston, *for ascertaining the Refracting Indices of Bodies. By* John Thomas Cooper, *Esq.**

THE ordinary physical characters of substances such as hardness, colour, lustre, fracture, specific gravity and some others, have been the means which chemists and mineralogists have long been in the habit of employing for the identification both of inorganic and organic substances; and I think it will be acknowledged by all who give their attention to such matters, that any additional means added to the above modes of observation, if it be capable of being put into practice with equal facility, and with a certainty of giving results with a degree of precision little, if at all inferior to that which is capable of being attained by the balance, is a sufficient apology for occupying a short time of the Chemical Society.

About forty years ago, the late Dr. Wollaston described in the Philosophical Transactions, an ingenious instrument by which the refractive indices of substances, either in a solid or liquid state, could be with facility determined, the method re-

* Communicated by the Chemical Society; having been read April 18, 1843.

quiring extremely small quantities of the matter to be subjected to experiment, was an additional recommendation for its adoption in practice, but it has never been generally brought into use; and the cause which in my opinion has operated more than any other to prevent its more extended employment in the laboratory of the chemist, is the limited extent of substances which, in the form he gave it to the world, could by its means have their indices of refraction determined with accuracy. It is with the view of rendering the method of Dr. Wollaston more generally known, and of putting the Society into possession of a knowledge of some alteration in the construction of the instrument, by which it is rendered of a more extended application, that I have ventured to call the attention of the Society to this subject.

Instead of employing only one species of glass as recommended by Dr. Wollaston, I make use of several, each of which is suited to the nature of the substance under examination, and that species of glass is selected for the purpose, which, when the substance to be examined is applied to the base of the prism, gives with the subject so applied, when viewed in its position, neither too acute or too obtuse an incidence; for it is at extreme incidences, as far as I have been able to observe by the means here employed, that erroneous results are liable to be obtained. The glass prisms which I am in the habit of using have respectively a refractive index for Fraunhofer's ray *b* of

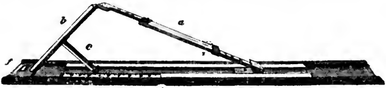
1·516, which is ordinary plate glass.

1·583, which is common flint glass.

1·635, a very heavy flint glass, which I made some years ago for optical purposes.

1·816, Faraday's borate of lead glass.

Now in order that these different prisms may be used, a modification of the apparatus as originally proposed by Dr.



Wollaston is requisite, or otherwise each prism will require a separate and distinct instrument; but to accomplish this object with but one instrument, I have changed the position of the indicator from the longer of the bars to the shorter, and constructed the longest bar *a* in such a manner that it is capable of being extended in length from 15·16 to 18·16 inches,

while the height of the shorter bar *b* retains the original length proposed by Dr. Wollaston of ten inches, and the indicator *c*, as a consequence, has only the half of this length, that is to say five inches. The bottom bar *d* is about two feet in length, and has a dove-tailed groove or furrow ploughed in it throughout its whole length; in this groove a piece *e* is made to slide easily, which may be clamped or made fast by means of a thumb-screw in any part that may be required. To this sliding piece is attached a hinge, and to this hinge is also attached one of the sliding bars 1, the other sliding bar 2 being hinged to the bar *b*. These sliding bars are capable of being fixed at any required length, by means of a clamping-screw. The bar *b* is hinged to an immoveable block *f* of about two inches square, having an excavation of about three quarters of an inch square formed in it, for the purpose of preventing the substances submitted to experiment, on being placed on the base of any one of the prisms, from coming into contact with the wood of which the block is made. Exactly in the middle of the bar *b* is hinged the indicator *c*, which is of brass and is filed to a very sharp edge; it is precisely five inches in length from the centre of the hinge to its extremity. This indicator may be slid along the graduated scale *g* which is laid down on the upper surface of the bottom bar *d*, and by means of the pressure of the short bar *b* to which it is attached, will remain in close contact with the graduated scale in whatever position the bars are capable of being placed; the sharp edge of the indicator is always perpendicular (provided the lengths have been duly attended to, by which is meant the precise distances of the centres of motion of the hinges from each other) to the axis of the hinge which connects the bar *b* with the moveable bar *a*.

It now only remains to be stated in what manner the instrument is to be adjusted, which is to be effected in the following manner:—When a piece of glass has been selected that is capable of giving with the substance under examination a total reflexion at a mean incidence (by which is meant, when the total reflexion occurs at an angle varying from about 35° to 65°), supposing this to be the case with a prism whose refractive power is 1·635, such as would be required to obtain the refractive power of any or most of the fixed oils, then the adjustable bar is to be made sixteen inches and thirty-five hundredths of an inch in length between the centres of the pins of the hinges, and the bottom bar to be shifted until the brass edge of the indicator stands at ·635 on the scale; the substance on being applied to the base of the prism is then to be put into its situation on the block, and the whole ap-

paratus placed in such a way that the light from the sky may fall and be reflected from the base of the prism.

The eye being directed along the upper edge of the short or ten-inch bar, the latter is to be elevated or depressed just until the faintest gleam of the substance is to be seen in the bright light reflected from the prism's base, and which, if properly managed, will appear of a very pale blue or bluish-green colour; when this occurs the indicator will point out the refractive power of the substance under examination. If a very volatile substance, such as any of the æthers or hydrocyanic acid, should be the subject of experiment, I then am in the habit of employing a small piece of flat glass of a dark colour attached by means of the fluid, to be examined, with a very slight pressure to the base of the prism; this will effectually prevent evaporation of the fluid for a period of sufficient duration to enable any one with ease and precision to determine the refractive index or power of such a substance; and in general I prefer using this for all liquids, as it permits a more extended and uniform surface of the matter under examination, and diminishes the liability to error.

LXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 472.]

June 29, 1842. 3. "NOTICE on the Discovery of Insects in the Wealden of the Vale of Aylesbury, Bucks, with some additional observations on the wider distribution of these and other Fossils in the Vale of Wardour, Wiltshire." By the Rev. P. B. Brodie, F.G.S.

In a former notice (Phil. Mag. S. 3. vol. xv. p. 534) Mr. Brodie announced the discovery of insects as well as a new genus of Isopods in the Wealden beds of the Vale of Wardour, and in this communication he gives an account of additional localities in the same Vale, where he has found both the insects and crustaceans, and of the strata belonging to the Wealden series, in which he has obtained fossil insects, in the Vale of Aylesbury.

Vale of Wardour.—The precise spot noticed in the former paper is a quarry at Dallards, and the first point to which the author now calls attention, is situated about two miles to the south-east of it. The following section is given of the beds at the new locality, the dip being slightly to the south:—

	ft.	in.
1. Top. Debris of rounded fragments of greensand and Portlandstone, with their usual fossils, a few inches thick.		
2. Chert, full of Cyclas; it also contains occasionally Bufonites	1	6

	ft.	in.
3. Hard, brownish white limestone, with Ostreæ and casts of other shells, some resembling those of *Cyclas major*. The upper layers much disturbed	2	0
4. Black earthy clay, a few inches.		
5. Purbeck stone, varying in character but containing Cyclades..................................	5	0
6. Fissile, soft stone full of Modiolæ, palates and other remains of fishes, also bones of a species of tortoise	1	0
7. White limestone, containing Isopods and elytra of Coleoptera................................	3	0
Hardstone.		

In an escarpment in the banks of the adjoining river are two beds of limestone, from the upper of which Mr. Brodie obtained small elytra, and from the lower Cypris, and from both carbonized wood, also a species of Cyclas. Under these strata is a very oolitic limestone, in which the author found a small Melanopsis and a seed-vessel.

A mile distant Mr. Brodie procured from a bed of limestone, about five inches thick, Cyclades, Isopods, and a small fish of the species which occurs at Dallards; and in a bed of clay, bones of a tortoise. The hard crystalline limestone of the Lady-down beds are noticed as yielding, but rarely, Cyclades and Cyprides. In the neighbourhood of Tisbury, in a soft, gritty, slightly oolitic stone, the author found Isopods of a larger size than elsewhere, likewise an elytron of a coleopterous insect. Though the number of beds of limestone vary in different parts of the Vale of Wardour, yet Isopods and insects characterise the whole of them; and as respects lithological characters, notwithstanding the great varieties which occur at different localities, there is throughout the district that general peculiarity of aspect which is so remarkable in freshwater formations of very different ages, and which serves to identify detached quarries with each other.

Vale of Aylesbury.—In Buckinghamshire the Wealden beds possess a certain similarity with those in Wiltshire, but with clearly marked local differences. At Quainton Hill Mr. Brodie could not discover any traces of fishes, insects, or Isopods. In a quarry near the village of Stone he obtained the following section:—

1. Rubble, several feet.
2. Hard white stone, no fossils .... 2 to 3 feet.
3. Greenish stone, with Cypris................ 2 feet.
4. Black clay, containing bones of a Tortoise .... 1 foot.
5. White and blue limestone (Pendle), yielding Modiolæ in abundance; also a few Cypris and Cyclas; likewise bones and palates of fishes, coprolites, and, but rarely, remains of insects; fragments of carbonized wood are common; and Mr. Brodie obtained a specimen of *Sphenopteris Mantelli*, and another minute but beautiful species of Fern. This limestone bears a close resemblance to one of the beds at Dallards.

In his general observations on the fossils from these different localities, the author states, that though he has greatly added to the number and variety of insect-remains since his former communication, yet

he has not found any of the larger kinds, almost every specimen requiring a high magnifying power to be seen distinctly. Next to the Coleoptera, the most prevalent orders are the Homoptera and Trichoptera; and Mr. Brodie observes, that this fact accords with the habits of the two latter orders, the first living on plants, remains of which are found abundantly in the Wealden, and the second hovering over the surface of streams. From the fragmentary state of these remains, and from the wings never being expanded in the more nearly perfect specimens, he considers it probable, that they were carried for some distance down the streams which flowed into the Wealden estuaries. A few of the insects which have been examined by an eminent entomologist, have been pronounced to possess, with one exception, a decidedly European character, to differ from those at Aix, and to be less tropical than those found at Stonesfield.

Since the reading of his prior communication, Mr. Brodie has obtained Isopods an inch and a half in length and an inch broad. These crustaceans, so interesting from the analogy to Trilobites, presented by allied genera, are rarely found in single specimens, but in groups, and therefore present this additional agreement with the habits of recent species. The fossils appear to have been deposited tranquilly at the bottom of the water which they inhabited, being always found imbedded with their legs downwards, and they are generally well-preserved. The whole of the freshwater remains of these Wealden beds, including the testacea, afford the natural characters of such deposits by yielding abundance of specimens, but few genera.

Associated with the above-mentioned organic remains of the Vale of Wardour, Mr. Brodie has obtained three species of small fishes quite distinct, he says, from those found at Lady Down and Chicksgrove. With a single exception they were all procured at one spot.

None of the localities mentioned in the paper afforded the least trace of the "dirt-bed," or of Cycadeoidea.

4. "On the Geology of Egypt." By Lieut. Newbold of the Madras Army, F.R.S. [An abstract of this paper will be found in our preceding volume, p. 215.]

5. A letter, addressed to the Secretaries by C. Kaye, Esq., "On a Collection of Fossils discovered by the writer in Rocks in Southern India."

The localities from which Mr. Kaye procured his suites of specimens are Pondicherry, Trichinopoly, and Verdachellum.

Pondicherry.—From a limestone in the neighbourhood of this city, Mr. Kaye obtained Nautili in great abundance, belonging to at least three species; Ammonites in even greater numbers and well-preserved, and although assignable to thirteen distinct species, the author has not been able to identify a single specimen with any European Ammonites of which he has seen a description. Baculites likewise occur in such quantities as often to constitute the entire mass of large blocks; and Hamites in a great variety of forms, besides numerous genera of conchifera and mollusca; likewise Echinidæ, Polyparia, fishes' teeth, and considerable masses of calcareous wood bored by Teredines.

All these fossils were discovered by Mr. Kaye and a friend within the last two years, and are entirely new to European palæontologists.

In the neighbourhood of Pondicherry and bordering on the limestone is a bed of red sand containing an immense quantity of the silicified wood long known to collectors.

Trichinopoly.—The spot in this district from which Mr. Kaye procured his specimens he was not able to visit. The fossils occur also in a limestone, preserve their shelly matter with occasionally the colour, and belong principally to marine genera, but some are considered to be of freshwater origin. Cephalopods appear to be of very rare occurrence, Mr. Kaye having obtained from the locality only one fragment of a large Ammonite. Wood bored by Teredines is also found in the limestone.

Verdachellum.—From a calcareous rock near Verdachellum, forty miles from Pondicherry, Mr. Kaye procured a variety of marine shells, including a considerable number of Ammonites, considered by him to be distinct from those found near Pondicherry; also a few imperfect Nautili and a few Echinidæ, corals, &c.

Among the testacea are several considered to belong to species found in the Trichinopoly deposit, and a few believed by Mr. Kaye to be identifiable with Pondicherry shells. This limestone is likewise bordered by a red sand which contains specimens of silicified wood. The formation was discovered only a short time before the writer quitted India, and he consequently considers his collection as defective; but he regards the deposit whence it was obtained as of interest, affording, by its position and organic contents, a link between the other two localities.

6. A paper "On the Fossil Foot-prints of Birds and Impressions of Rain-drops in the Valley of the Connecticut." By Charles Lyell, Esq., V.P.G.S.

The deposit in which these impressions, long known on account of the researches of Prof. Hitchcock, occur, is situated in a trough of hypogene rocks, about five miles broad, the strata, which consist of sandstone, shale and conglomerate, dipping uniformly to the east at angles that vary from 5° to 30°. Mr. Lyell first examined the red sandstone at Rocky Hill, three miles south of Hartford, in Connecticut, where it is associated with red shale and capped by twenty feet of greenstone. Many of the beds are rippled, and cracks in the shale are filled by the materials of the superincumbent sandy layer, showing, the author observes, a drying and shrinking of the mud while the accumulation of the strata was in progress. The next quarries he examined were at Newark in New Jersey, about ten miles west from New York city. The excavations are extensive, and the strata dip, as is usual in New Jersey, to the north-west, or in an opposite direction to the inclination in the valley of Connecticut, a ridge of hypogene rocks intervening. The angle is about 35° near Newark. The beds exhibited ripple-marks and casts of cracks, also impressions of rain-drops on the upper surface of the fine red shales. Mr. Lyell states, that he felt some hesitation respecting the impressions first assigned to the action of rain by Mr. Cunning-

ham of Liverpool*, but he is now convinced of the justness of the inference, having observed similar markings produced on very soft mud by rain at Brooklyn in Long Island (New York). On the same mud were the foot-prints of fowls, some of which had been made before the rain and some after it.

Mr. Lyell next visited the red and green shales of Cabotville, north of Springfield in Massachusetts, where some of the best Ornithichnites have been procured, chiefly in the green shale. The dip of the beds is 20° to the east, a higher inclination, the author says, than could have belonged to a sea-beach. He observed in the same quarries ripple-marks as well as casts of cracks, and he was informed that the impressions of rain-drops have likewise been found.

In company with Prof. Hitchcock, Mr. Lyell afterwards examined a natural section near Smith's Ferry, on the right bank of the Connecticut, about eleven miles north of Springfield. The rock consists of thin-bedded sandstone with red-coloured shale. Some of the flags are distinctly ripple-marked, and the dip of the layers on which the Ornithichnites are imprinted, in great abundance, varies from eleven to fifteen degrees. Many superimposed beds must have been successively trodden upon, as different sets of tracks are traced through a thickness of sandstone exceeding ten feet; and Prof. Hitchcock pointed out to the author that some of the beds exposed several yards farther down the river, and containing Ornithichnites, would, if prolonged, pass under those of the principal locality, and make the entire thickness throughout which the impressions prevail, at intervals, perhaps twenty or thirty feet. Mr. Lyell, therefore, conceives that a continued subsidence of the ground took place during the deposition of the layers on which the birds walked.

It has been suggested, but the opinion has not been adopted by Prof. Hitchcock, that the eastward slope of the beds represents that of the original beach. With a view to this question, Mr. Lyell examined the direction of the ripple-marks, and found that it agreed with the dip, or was at right angles to the supposed line of beach; but he adds, though this agreement presents a formidable objection to the suggestion above alluded to, if the ripples were produced by waves, yet it does not disprove the opinion, as the ripples do not exceed in dimensions those which are produced by sand blown over a muddy beach, and often distributed at right angles to the coast-line. Instances of this effect of the wind Mr. Lyell has remarked along the shores of Massachusetts. Nevertheless he is of opinion that the rippled layer of sandstone in question contains too much clay to have resulted from blown sand, and he is disposed to think that in most of these localities the strata have been tilted, instances of such disturbance having been pointed out to him by Prof. Hitchcock in the state of Massachusetts, and by Mr. Percival near Newhaven in Connecticut. In reference to this subject, he says, that a few miles from Smith's Ferry a conglomerate, several hundred feet thick, containing angular and rounded fragments of trap and red sandstone, the base being sometimes a vesicular trap and trap tuff, passes upwards into

* See Phil. Mag. S. 3. vol. xiv. p. 507.

the very flags on which Ornithichnites occur; and from this he infers, that there were eruptions of trap, accompanied by upheaval and partial denudation, during the deposition of the red sandstone.

With respect to the impressions having been made by birds, Mr. Lyell states, that until he examined the whole of the evidence he entertnined some scepticism, notwithstanding the luminous account given by Prof. Hitchcock. In proof of their being the foot-prints of some creature walking on mud or sand, he mentions, 1st, the fact of Prof. Hitchcock's having seen 2000 impressions, all, like those he had himself examined, indented in the upper surface of the layer, the casts in relief being nlways on the lower surface; and 2ndly, that where there is a single line of impressions the marks are uniform in size, and nearly uniform in distance from each other, the toes in the successive steps turning alternately right nnd left. Such single lines, Mr. Lyell says, indicate that the animal was a biped, and the trifid marks resemble those which a bird leaves, there being generally a devintion from a straight line in any three successive prints; and his attention having been cnlled to indications of joints in the different toes, he afterwards clearly recognised similar markings in the recent steps of coots nnd other birds on the sands of the shores of Massachusetts. Prof. Hitchcock has shown, thnt the same impression extends through several laminæ, decreasing in distinctness in proportion as the layer recedes from that in which it is most strongly marked, or in proportion as the sediment filled up the hollows and restored the surface to a level; and Mr. Lyell states, that he has observed a great number of instnnces of this fact.

He also says, that he can scarcely doubt that some of the impressions on the red sandstone of Connecticut are not referable to birds, but he believes that the gigantic ones described by Prof. Hitchcock are Ornithichnites. At Smith's Ferry they nre so numerous that a bed of shale many yards square is trodden into a most irregular and jagged surface, so that there is not a trace of a distinct footstep; but on withdrawing from this area to spots where the same tracts are fewer, the observer, Mr. Lyell says, is forced to admit that the effect in each case has been producd by this cause.

On examining the shores on some small islands about fifteen miles south-east from Savannah, the author was struck with the number as well as the clearness of the trncks of raccoons and opossums imprinted in the mud during the four preceding hours, or after the tide had begun to ebb. At one spot, where the raccoons had been attracted by the oysters, the impressions were as confused as when a flock of sheep has passed over a muddy road; and in consequence of n gentle breeze blowing parallel to the line of cliffs composed of quartzose sand, the tracks had in mnny places already become half-filled with blown sand, and in others were entirely obliterated; so that if the coast should subside, the consolidation of this sand would afford casts analogous to those of Storeton Hill in Cheshire, yet the impressions had been made and filled in a few hours.

When considering the broad question whether the fossil foot-prints were made by creatures walking on mud or sand after the ebbing of

the tide, Mr. Lyell reminds his readers of the fact that in the United States, as in Saxony nnd Cheshire, the tracks in sandstone and shale are accompanied by littoral appearances, as ripple-marks, the casts of cracks in the clay, and often by the marks of rain.

In regard to the age of the red sandstone of the valley of the Connecticut and New Jersey, the author states he has nothing to add to what had been previously advanced, by which its position had been shown to be between the carboniferous and cretaceous series. In the neighbourhood of Durham, Connecticut, he had collected in the sandstone, fishes of the genera Palæoniscus nnd Catopterus, but no other organic remains, except fossil wood.

In conclusion, Mr. Lyell remarks, 1st, that the Ornithichnites of Connecticut should teach extreme caution in inferring the non-existence of land animals from the absence of their remains in contemporaneous marine strata; 2ndly, that when this red sandstone of Connecticut was deposited, there was land in the immediate vicinity of the places where the Ornithichnites occur; and that but for them it might naturally be inferred that the nearest land was several miles distant, namely, that of the hypogene rocks which bound the basin of the Connecticut. Now, the land that caused the sea-beach, Mr. Lyell says, must have been formed of the same sandstone which was then in the act of accumulating, in the same manner as where deltas are advnncing upon the sea.

In a postscript, Mr. Lyell states, that subsequently to writing the paper he had read the luminous report of Mr. Vanuxem on the Ornithichnites described by Prof. Hitchcock, and though it agrees in substance with his own account in some particulars, yet that he has left his notice as it stood.

7. The following notice by Captain Pringle respecting the Ochil Hills:—

A gentleman resident in the district had often remarked the occurrence of sounds, which appeared to him to be subterranean, but which the country people attributed to noises from the river Divan, or to the machinery of iron-works some miles distant. At the time of the earthquake, however, which was felt at Comrie in October 1840, he was on the hill and heard a loud noise like the rushing of steam through a cavern, and the same noise was heard also by others two to three miles distant. On inquiry he ascertained that the noise was contemporaneous with the earthquake, and that the machinery at the iron-works was at that moment not in action.

The Gaelic word *ochain* or *ochail* signifies moaning, howling, wailing (Armstrong's Dictionary); and hence it is inferred that the name of the "Moaning Hills" may have been given to the range from the sounds so frequently heard in the district; and further, that the sounds are connected with the earthquakes felt in the neighbourhood, near Crief and Comrie. [On these Earthquakes see Phil. Mag. S. 3. vol. xx. p. 240.]

November 2, 1842.—On the Geology of the Western States of North America. By David Dale Owen, M.D., of Indiana*.

* Abstracts of this and of other papers on the Geology and Palæonto-

Nov. 16, 1842.—A paper was read " On the Structure of the Delta of the Ganges, exhibited by the Boring Operations in Fort William, A.D. 1836–40." By Lieut. R. Baird Smith, B.E.

Since the year 1804, a number of boring operations have been conducted in the Gangetic Delta, with a view to supply the deficiency of good fresh water in the vicinity of Calcutta, but, from mechanical obstacles, without success. The geological results of the last of these experiments, commenced in April 1836, and abandoned in 1840, after being carried on to the depth of 480 feet, are detailed by Lieut. Smith in this memoir. After penetrating to the depth of ten feet through the artificial surface soil, a bed of blue clay, close and adhesive in its texture, was entered. As the bore descended, the clay became darker in colour, till, in from thirty to fifty feet, large portions of peat, with decaying fragments of trees, were found. These Dr. Wallich identified with the common *Soondri* of the Sunderbunds, and the roots of some climbing tree resembling *Brædelia*. The stratum of peat and decayed wood was therefore formed from the debris of forests which at a former period covered the entire surface of the Delta, as the existing jungles of the Sunderbunds cover so large a portion of it now. In one instance bones were found in the peat, but they were unfortunately destroyed by the workmen before examination. Succeeding these peat-charged beds, a stratum of calcareous clay, ten feet in thickness, is found, and intermixed with it are portions of the concretionary limestone, commonly known in India as kankur, which Lieut. Smith regards as formed by the segregation of the particles of calcareous matter disseminated throughout the body of the clay with which it is associated, and as nearly contemporaneous in its origin with this clay. Underlying the bed of calcareous clay in which the kankur first occurs, there is a thin bed of green siliceous clay, extending from sixty to sixty-five feet in depth. The clay then loses its colour, and continues to a depth of seventy-five feet, the lower portion of it furnishing nodules of kankur. At seventy-five feet, a bed of variegated, sandy, or arenaceous clay commences, and continues to the depth of 120 feet, occasionally traversed by horizontal beds of kankur. Beneath this is a stratum of argillaceous marl, five feet in thickness; and succeeding it there is a bed only three feet in thickness, of loose friable sandstone, the particles of sand being held loosely together by a clayey cement. Argillaceous marl, twenty feet in thickness, follows the sandstone, terminating at the depth of 150 feet, when it passes into an arenaceous clay, intermixed with water-worn nodules of hydrated oxide of iron. Weathered mica slate is found attached to the clay of this bed, and throughout the entire range of strata penetrated, scales of mica have always been abundantly met with. At 175 feet, a coarse friable quartzose conglomerate occurs, composed of pebbles of different sizes, though none are very large, cemented together by clay. At 177 feet, this conglomerate becomes smaller grained; and at 183 feet 3

logy of North America, by Dr. Dale Owen, Mr. Lyell, Dr. Mantell, Mr. Redfield and Mr. Cooper, read before the Geological Society from Nov. 2, 1842, to Feb. 1, 1843, have been given in the present volume, p. 180.

inches, it is found to pass into indurated ferruginous clay, which continues, with but little variation, to a depth of 208 feet. Here another layer of sandstone, soft in its upper portion, but becoming more indurated, and assuming the lamellar structure as it is passed through, occurs; the thickness being, however, no more than three feet. Ferruginous sand, with thin beds of calcareous and arenaceous clay, prevail from 208 feet to 380. Kankur, with minute water-worn fragments of quartz, felspar, granite, and other indications of debris from primary rocks, are met with in the lower parts of this sandy deposit, in which were also found three fragments of bones, of which one was considered by Mr. J. Prinsep to be the lower half of a humerus of some small quadruped like a dog, and another the fragment of the carapace of a turtle. At 380 feet, there occurred a thin layer, only two feet in thickness, of blue calcareous clay, thickly studded with fragments of shells; and at 382 feet, this was succeeded by a layer of dark clay, composed almost entirely of decayed wood. From the lower portion of it several fragments of coal, of excellent quality, were brought up. Underneath this stratum, and in the gravelly bed which immediately succeeds it, there were found several other fragments of fossil bones. One was considered to be a caudal vertebra of a kind of lizard, and the rest were fragments of turtles. These were discovered at the depth of 423 feet, and were associated with large rolled pebbles of quartz, both white and amethystine, felspar, limestone, and indurated clay. The gravel, composed entirely of the debris of primary rocks, continued to the depth of 481 feet, where the operations ceased.

The fossils recorded above, observes the author, were found in two distinct deposits, separated from each other by the interposition of a bed of shelly, calcareous clay and a deposit of carbonaceous matter ten feet in thickness, the remnants of some extensive forest which flourished at a period anterior to the deposit of the 380 feet of superincumbent sands and clays. The lithological characters of the superior and inferior fossiliferous deposits differ considerably from each other, the former being a fine and slightly indurated sandstone, the latter a coarse conglomerate, formed of the debris of primary rocks, imbedded in an arenaceous matrix. The fossils of the upper bed, which is about eighty feet in thickness, furnish the only specimens of mammalia obtained during the operations. These were associated with the remains of Chelonians, but no indications of the existence of saurian animals were discovered till the shelly clay and carbonaceous bed were passed through, and from the lower conglomerate no mammalia were obtained. In drawing any conclusions, however, the limited space examined, the diameter of which was not more than six inches, must be borne in mind.

Lieut. Smith remarks the correspondence of the succession of the strata in the Gangetic Delta, at a depth of from 350 to 480 feet, with that observed by Captain Cautley at the base of the Himalaya.

The nature of the fossil remains and the dimensions of the gravel found at 480 feet from the surface of the ground, the greatest depth hitherto attained, were such as to lead Dr. M'Clelland to the con-

clusion, that when these were originally deposited bold rocky mountains existed in close proximity to the present site of Calcutta; and taking his data from the results of personal observation on the transporting power of rapid currents, he estimates the distance of these mountains at not greater than twenty or thirty miles. Resting on the bed of coarse conglomerate, the entire depth of which is unknown, although it cannot be less than eighty feet, the bore having pierced it to that extent, there are beds of carbonaceous matter and lacustrine clay bearing the clearest evidence of having been quietly deposited on a marshy surface clothed with vegetation. Ere this could have taken place, the powerful currents indicated by the gravel must have been arrested, and as this could only be effected by a great lowering of the inclination of the bed of the river, we may infer the check arose from the entire subsidence of the range of hills above alluded to. The extent to which this took place it is impossible for us to estimate, but the deposits which the river continued to make would repose upon the depressed masses, and were boring operations to be carried on successfully in such localities they would ultimately expose these again to our observation. Supposing then, as without impropriety we may do, that the rocks of which these hills were composed stretched away beneath the conglomerate bed formed by the large gravel borne along by the torrent issuing from them, we are led to believe that had the Fort William boring operations been successfully carried through the entire depth of the conglomerate, the auger would then have impinged on the solid rock, and if so, would the experiment have terminated favourably?

"When we remember," observes Lieut. Smith, "that the conglomerate was almost entirely composed of debris from primary rocks, admitting of the inference that the chain of hills itself was formed of members of this series, there can be but little hesitation in replying in the negative."

"On Pipes or Sandgalls in Chalk." By Joshua Trimmer, Esq., F.G.S.

In a former paper (Proceedings, vol. iii. p. 185) the author described two detrital deposits in Norfolk, which appear to have been produced by powerful currents of water. The lowest of these is marked on the surface with numerous furrows and penetrated by cylindrical and funnel-shaped cavities like those of the chalk, though in general of smaller dimensions. If these have been caused by the mechanical action of water, they indicate a pause between the two deposits of sufficient duration to allow of the consolidation of the lower bed before the other was thrown down upon it. Therefore, to learn the true history of the beds, we must discover the cause of the pipes; the action is so similar in the chalk and the detrital deposits that the one will explain the other.

From recent study of the pipes or sandgalls in the chalk of a part of Kent, Mr. Trimmer has arrived at the conclusion that they are due to the mechanical, not to the chemical action of water; and that this action was the breaking of the sea on a low shore antecedent to the formation of the eocene strata. This opinion he bases on the following grounds.

Having had an opportunity of observing the removal of its covering from the chalk near Faversham, Mr. Trimmer found that the pipes were but the terminations of furrows from six to twenty-four inches deep in the shallowest parts exposed, but widening and deepening as they approached the pipes till they were lost in them.

The diluvial covering spread over the chalk is a strong loam of a reddish brown colour, with numerous unabraded flints dispersed through it. The pipes were filled with loam of a more sandy nature and of a much lighter colour. The few pebbles found in them consisted of chalk flints much water-worn, and contrasting strongly with the unabraded flints of the diluvium. Their sides were lined with clay, tinged black. The lower part of the diluvial deposit, near its junction with the chalk, had in many places the same black tints. None of the pipes terminated downwards in a point, the apices of the inverted cones being three or four inches broad. These facts the author considers indicative of the mechanical action of water.

He observed certain blocks of siliceous sandstone, derived from the sands of the London clay, marked with similar pipes and furrows, though of smaller dimensions, which could not have been formed by the action of acidulated water. In these the pipes occasionally commenced from the opposite sides of the same block, perforating it, therefore not formed by rain. On the sea-shore near Reculver he saw similar blocks, presenting pipes in miniature. The waves charged with small pebbles and sand, wearing the surface with furrows like those of the chalk, the softer parts of the stone then giving way, first hollows are formed, then the rotatory motion of the contents of the hollows, set in action by the influx and reflux of the waves, drills the pipe.

The pipes and furrows in the sandstone blocks Mr. Trimmer considers as having been produced by the same agency, and their perforation, as caused in consequence of their reversion by a violent storm and the drilling operation then going on at the opposite side.

The examination of a chalk bed near Canterbury convinced Mr. Trimmer that the same causes had produced the pipes and furrows in the chalk. He remarks, that the sand with which the pipes were filled contains much calcareous matter, and that it appears impossible that acidulated water, percolating from above, could have acted on the chalk without first removing all carbonate of lime from the sand.

In all cases observed by Mr. Trimmer the sandgalls were confined to the edges of channels which are either now traversed by tidal currents like the trough of the Thames, or appear, like the dry combes, to have communicated with the sea at some remote period.

From the above facts, Mr. Trimmer infers that the pipes in the chalk of the part of Kent examined were formed by the action of the sea on a low shore; that they mark the boundaries of the ante-eocene sea, and that they were subsequently submerged and covered by the London clay.

"On some remarkable Concretions in the Tertiary beds of the Isle of Man." By H. E. Strickland, M.A., F.G.S.

The north extremity of the Isle of Man consists of an arenaceous pleistocene deposit, occupying an area of about eight miles by six,

bounded on the west, north and east by the sea, and on the south by the mountains of Cambrian slate which occupy the greater portion of the island. The arenaceous formation attains in some parts a height of about 200 feet above the sea, though the undulations of its surface prove that considerable portions of the deposit have been removed by denudation. This district, comprising about fifty square miles, furnishes perhaps the most extensive example in the British Isles of a marine newer pliocene or pleistocene deposit. In the Isle of Man the sea-cliffs on each side of this tertiary district afford a good insight into its structure and composition. On the north of Ramsey the cliffs average about 100 feet in height, and consist principally of irregularly stratified yellowish sand, sometimes clayey, with interspersed bands of gravel and scattered pebbles. The gravel is chiefly composed of slate-rock, quartz, old red sandstone, granites, porphyries and chalk flints, all of which occur *in situ* in the island except the last two, which may have been drifted, the former from Scotland, and the latter from the north of Ireland. About four miles north of Ramsey the cliffs attain 150 feet. Here the lowest portion, only visible at intervals, is a brownish clay loam, and the remainder of the cliff is sand and coarse gravel, less distinctly stratified than is the case near Ramsey, and containing rudely rounded boulders, some of which are upwards of a ton in weight. They consist of granite, and occasionally of carboniferous limestone.

Organic remains are sparingly diffused in this deposit: Mr. Strickland enumerates twenty species. Of these five, viz. *Crassina multicostata, Natica clausa, Nassa monensis, Nassa pliocena*, and *Fusus Forbesi* are not known in the British seas. *Crassina multicostata* and *Natica clausa* are found living in the Arctic ocean, but the two species of *Nassa* and the *Fusus* are unknown in a recent state*.

* Mr. Strickland gives the following characters of three species of shells found in the newer pliocene beds of the Isle of Man; specimens of which have been examined by several eminent conchologists in London, who all concur in believing them to belong to extinct species.

"1. *Nassa monensis*, Forbes, in Mem. Wern. Soc., vol. viii. p. 62. Small; volutions about six, rounded; suture deep; ribs, nine on the first volution, straight, rather distant, strong, subacute, and slightly oblique. The first volution has thirteen, and the second six, distinct, regular, thread-like, spiral striæ, crossing alike the ribs and their interstices. Aperture orbicular-ovate, canal very short and oblique, pillar-lip simple, outer lip with about five slight marginal denticles on the inside, and an external rib slightly more developed than the ordinary ribs. Total length, 7 lines; first volution, 3¼ lines; breadth, 4¼ lines; angle of spire, 40°.

"*Obs.* Resembles the recent *N. macula*, but is larger, more ventricose, has fewer ribs, and the terminal rib is less suddenly developed.

"2. *Nassa pliocena*, Strickland, 1843. Large; volutions about seven, rather flat, with a distinct thread-like suture; ribs, twelve on the first volution, straight, distant, rounded, very slightly oblique; the interstices flat, exceeding the width of the ribs by one-half. The first volution with thirteen, and the second with about nine fine spiral striæ, only visible in the interstices, the ribs being smooth; but this may be due to attrition. Aperture ovate; canal very short and oblique; pillar-lip with about five obscure denticles, and a spiral groove immediately behind the canal, continued into the

Between three and four miles north of Ramsey, the beds of this deposit occasionally exhibit a very remarkable concretionary structure. The sand has here been cemented into masses, which are extremely hard, and even sonorous when struck, though the sand in which they are imbedded is perfectly loose. The cementing ingredient, which the application of acid proves to be carbonate of lime, seems to have been influenced in its operations partly by the planes of stratification, and partly by the direction in which the sand has been originally drifted by currents. In the former case the concretions are in the form of flat tabular masses parallel to the stratification, often mammillated on their surfaces, or perforated obliquely by tubular cavities. In the latter case they assume a subcylindrical or spear-shaped form, and occur parallel both to the stratification and to each other. A pebble is frequently attached to the larger end of the concretion, which springs from it as from a root, to the length of a foot or more, and gradually terminates in an obtuse flattened point. All these varieties are sometimes combined together into vast clusters of several tons weight, resembling masses of stalactite, the component portions being nearly parallel to each other. Mr. Strickland supposes that currents of water (or possibly of wind, operating during ebb tide), flowing in a certain direction, may have disposed the sand in ridges parallel to that direction, and the carbonate of lime may have afterwards been attracted into these ridges in preference to the intermediate portions. This view is confirmed by the fact, that these concretions have frequently a pebble attached to the larger end, as though it had protected a portion of sand from the current, and caused it to accumulate in a ridge on the lee side, a circumstance which may frequently be observed where sand is drifted by the wind or water.

Nov. 30, 1842.—"On the Bala Limestone." By Daniel Sharpe, F.G.S.

interior of the shell. Outer lip with about eight internal marginal denticles; no rib at the back. Total length, 1 inch 8 lines; first volution, 8 lines; breadth, 9 lines; angle of spire, 40°.

"3. *Fusus Forbesi*, Strickland, 1843. *Fusus* nov. sp. Forbes, Malacologia Monensis, pl. 3. f. 1. Middle-sized; volutions about six, slightly rounded, suture distinct; ribs, eleven on first volution, straight, rounded, smooth (perhaps from attrition); interstices concave, and hardly wider than the ribs. First volution with about fifteen, and second with about seven distinct, rather irregular spiral striæ, of which those on the first volution are alternately large and small. They are only visible in the interstices of the ribs. Aperture ovate, double the length of the canal, which is straight, and rather oblique to the left. Pillar-lip smooth, with one obscure denticle at the posterior end. Outer lip with about ten small linear denticles within, continued a short way into the mouth, and a well-marked external rib remote from the margin. Total length, 1 inch 3 lines; first volution, 7 lines; breadth, 8 lines; angle of spire, 43°.

"*Obs.* This species belongs to a group of *Fusus* which seems closely allied to *Nassa*. First described by Mr. E. Forbes, from a worn specimen found on the coast of the Isle of Man, and supposed by him to be an existing species, but the discovery of additional specimens *in situ* proves it to be a genuine fossil."

Before entering upon his own views, the author quotes the opinions published by others upon the age of the limestones of Bala and Coniston; previous to the labours of Professor Sedgwick and Mr. Murchison, these two calcareous bands were thought to be of the same age, and to be nearly the oldest fossiliferous beds in this country; but the first definite arrangement of them was made by Professor Sedgwick, whose views will be found in our Proceedings (vol. ii. p. 675), placing both these limestones in the Upper Cambrian system, which he stated to lie below the Silurian system of Mr. Murchison, and above the Lower Cambrian system, or old slate series of Carnarvonshire, Cumberland, &c., a view adopted by Mr. Murchison in his work upon the Silurian system, upon the authority of Professor Sedgwick.

In 1839 Mr. James Marshall classed the Coniston limestone with the Caradoc sandstone, upon the evidence of fossils examined by Mr. J. Sowerby, and pointed out that it rested upon the Lower Cambrian rocks; thus omitting the Upper Cambrian system in the North of England (Reports of the British Association, vol. viii. p. 67.).

The second edition of Mr. Greenough's Map adopts Mr. Marshall's view of the age of the Coniston limestone, and omits the Upper Cambrians in the district of the Lakes; but retains them in North Wales, under the name of Upper division of the lower Killas, in which is included the Bala limestone, thus placed in a different system from the limestone of Coniston.

Professor Sedgwick's memoir of November 1841 follows the same view (Proceedings, vol. iii. p. 545); and in a note, p. 551, that author removes all doubt as to his opinions by apologizing for having formerly placed the Bala and Coniston limestones on the same parallel.

Notwithstanding the agreement of our best geologists in placing the Bala limestone in the Upper Cambrian system, Mr. Sharpe was induced to doubt the accuracy of this classification, by observing that everyone admitted that the Bala fossils agreed, as far as they had been examined, with those of the Lower Silurian beds, and that there was no clear line of separation between the Lower Silurian and Upper Cambrian groups: but his attention was particularly drawn to this district by Mr. Bowman's observations on Denbighshire, laid before the British Association in 1840 and 1841, and since published in the first volume of the Transactions of the Geological Society of Manchester, p. 194, which Mr. Sharpe regards as the first indication of the true structure of this part of North Wales; Mr. Bowman classes as Upper and Lower Silurian many beds before mapped as Upper Cambrian, showing that the previous classification of the rocks of North Wales could not be relied upon.

Mr. Sharpe quotes largely from Mr. Murchison's Address from the Chair in February 1842, to show that the Upper Cambrian cannot be separated from the Lower Silurian beds by the help of organic remains, as " Lower Silurian species range through the Upper Cambrian rocks, and throughout the whole of North Wales," and "prevailed during that vast succession of time which was occupied in the accumulation of all the older slaty rocks previous to the Upper Silurian period."

Mr. Sharpe points out, that up to the moment of his taking up the subject no one of the authors quoted had expressed a doubt of the existence of a great thickness of fossiliferous beds below the Caradoc sandstone and Llandeilo flags, although it was admitted that these supposed beds could not be distinguished by their fossils from the Lower Silurian; and he states that the object of his communication is to show the error of this view as relates to the Bala rocks, which he proposes to prove to be the equivalent of the Lower Silurian beds described by Mr. Murchison, and not part of an older series; and he infers from analogy that the same will be found to be the case in other parts of North Wales which he has not visited, where he conjectures that all the rocks containing shells of Lower Silurian species will also prove equivalents of the Lower Silurian beds. Instead of continuing the Silurian system downwards through a vast thickness of slate rocks, Mr. Sharpe proposes to strike out one of its original members, regarding the Caradoc sandstone and Llandeilo flags as one and the same formation which has received different names according to its mineral character; he observes, in confirmation of this view, that both formations are never equally developed in the same district, and that the fossils found throughout are too nearly the same to warrant the separation of the lower beds under a separate name. Still Mr. Sharpe believes that there are in Wales, as in Westmoreland and Cumberland, vast accumulations of slaty rocks below the Silurian system, in which no fossils have been found, and which must retain the appropriate name of Cambrian rocks.

Mr. Sharpe did not map the district in detail, but he traced two sections to show the position of the Bala beds with regard to the Berwyns, as he considered the question to turn upon the accuracy or error of the statement of Mr. Murchison, p. 308, "that the Bala limestone dips under the chief mass of the Berwyns."

The first section begins westward, at the igneous chain of Arenig Mawr, the natural boundary to the district; it crosses the town of Bala, and ends eastward at the Calettwr, where a dark slate, the upper bed of the Bala series, abuts unconformably against the clay-slate of Moel-halog, which is referred to the Cambrian system. This section places the Bala beds in a detached trough, and shows that they do not dip under the Berwyns: but their succession is not well shown, owing to the disturbed state of the surface.

The other section is in two parts; from the head of the lake of Bala up the Twrch to Bwlch y Groes, and across the Dyfi by Dinas Mowddy and Mallwyd, which line the author recommends to those who wish to study this series, as the rocks are well exposed in the upper part of the valleys of the Twrch and Dyfi: on the west it begins at the northern prolongation of the igneous chain of Arran Mowddy, and continues eastward through a conformable succession of beds up to the Upper Silurian; each section shows the whole of the Bala series, the upper bed of blue slate, which on the Calettwr rests unconformably against the Cambrian clay-slate, being the same which is overlaid conformably beyond Mallwyd by an Upper Silurian series of soft blue or liver-coloured shales alternating with hard, grey grits, without cleavage or fossils, dipping east-south-east, which Mr.

Sharpe identifies with the No. 2. of Mr. Bowman's lower division of the Upper Silurians, the probable equivalents of the Wenlock shale.

Mr. Sharpe then describes the Bala series of rocks, beginning with the uppermost beds.

1. *Dark blue slate.*—Worked at Craig Calettwr for good roofing-slates and flags; in one quarry the beds dip W.N.W 35°, and the cleavage planes dip W.N.W 65°; in another the beds dip W. 70° and the cleavage W. 80°. Between Dinas Mowddy and Mallwyd it is largely quarried for good slate and flags; the beds dip S.E. or E.S.E. about 30°; the cleavage is perpendicular, and strikes S.S.W. The lower beds pass into a soft argillaceous slate of no value. The whole is not less than 300 or 400 feet in thickness.

2. *Upper Bala limestone.*—A dark blue bed ten feet thick, accompanied by calcareous slates and soft brown shales, with many fossils, among which are *Orthis canalis* and *O. compressa*, and several new species. Mr. Edward Davis, who accompanied the author, discovered this bed at Pen-y Dall Gwm, four miles south-east of Bala, dipping W.½S. 70°: it is supposed to follow a line bearing N.N.E., much broken up by faults*.

3. *Rotten argillaceous schist and indurated shale.*—Light grey, weathering to brown, with many joints and few fossils; well exposed in the valley of the Dwm-lach, above its junction with the Dyfi: 400 feet thick.

4. *Bala limestone.*—A dark blue rock similar to No. 2, thirty or forty feet thick, with calcareous shales and grits full of organic remains, among which are *Orthis pecten, anomala, vespertilio* and *bilobata, Leptæna sericea, duplicata* and *depressa*, and *Spirifer radiatus*. This bed is much broken, and difficult to trace, but its general direction from Y-Garnedd, 1½ mile east of Bala, to the upper valley of the Cowarch, is nearly N.N.E. The line of limestone laid down, both in Mr. Murchison's and Mr. Greenough's Maps, is compounded of the beds No. 2. and No. 4.

5. *Grey slaty grits.*—Occasionally streaked or passing into brown, very hard; well seen on both sides of the lake of Bala and in the upper part of the valley of the Twrch; usual dip E.S.E. 45°, but much disturbed about the foot of the lake: the upper bed contains *Orthis canalis, anomala* and *vespertilio*. In the lower part is a bed thirty or forty feet thick of impure grey limestone with many fragments of *Trilobites* and other organic remains, among which Mr. Sharpe recognised *Bumastus Barriensis, Trinucleus Caractaci, Illænus crassicauda, Orthoceras approximatum*, and *Lituites cornu-arietis*. This bed was only seen near Rhiwlas and Llan-y-ci, on the north-west of Bala. The grits below the limestone are similar to those above, and contain *Orthis canalis* and *vespertilio, Leptæna sericea* and *Asaphus tyrannus*. The whole exceeds 500 feet in thickness.

6. *Rotten grey clay-slate*, weathering to brown, forming the moor between Bala and Arenig, and exposed where Cwm Croes joins the valley of the Twrch: supposed to be 500 feet thick.

* Mr. J. B. Morris has since met with the same bed in the valley of the Dyfi at Blaen-y-Pennant.

7. *Dark blue slate*, of poor quality, covers the eastern flanks of Arenig and Arran Mowddy, quarried at Blaen-y-cwm, where the beds dip N.E. 35°, and the cleavage dips E.N.E. 55°: the lowest bed of the series.

As the Bala beds are quite unconnected with the Cambrian rocks of the Berwyns, and are only overlaid by Upper Silurian deposits; as most of their organic remains are known Lower Silurian species, and as the total thickness of the whole series is about the same as has been assigned by Mr. Murchison to the Lower Silurians, Mr. Sharpe concludes that they are the exact equivalents of the Lower Silurian formation, and do not carry the series down below the beds described by Mr. Murchison. Mr. Sharpe considers it as easy to prove their identity with the Caradoc sandstone as with the Llandeilo flags, and again endeavours to show that these must be regarded as the same formation under different names. This classification replaces the dark blue limestones of Bala and Coniston, on the same parallel from which they were separated when Professor Sedgwick adopted Mr. Marshall's view of the Silurian age of the Coniston limestone, but left the Bala limestone in its erroneous position as part of the Upper Cambrians.

Mr. Sharpe adds comparative tables of the Silurian system as exhibited in three different districts:—in Westmoreland, as observed by himself; in Denbighshire and Merionethshire, the upper part taken from Mr. Bowman's memoirs, the lower added by himself; and in Shropshire, &c., as described by Mr. Murchison; but he defers the full comparison of these till he lays before the Society the conclusion of his remarks on Westmoreland.

Mr. Sharpe hopes that he has done away with an objection often made to the Silurian system, that it wanted a definite base, and was not distinctly separated from the Cambrian system; this was not overlooked by Mr. Murchison, who states that the line drawn between the two systems was provisional. The difficulty arose from classing with the Cambrian system many beds belonging both to the Upper and the Lower Silurians, and it will vanish when this is corrected; the lower boundary of the Silurian system will then prove as distinct in North Wales as in Westmoreland and Cumberland; but to produce this result, the country west of Llangollen and Welsh Pool must be remapped. Of the district now coloured as Upper Cambrian a small share will be given to the Ludlow and Wenlock formations, a larger portion to the Lower Silurians, and certain central bosses of older rocks will remain for the Cambrian system: but the Upper Cambrian of Professor Sedgwick, and its representative in Mr. Greenough's nomenclature, the upper division of the Lower Killas, must be struck out of our tables, and the Lower Silurians made to rest on the true Cambrian rocks.

The igneous rocks of Arenig and Arran Mowddy are described as varying compounds of felspar and quartz. The two chains bear nearly north, and their eruption is supposed by the author to have modified the face of the country, and to have caused much of its present complication, the prevailing strike previously having been N.N.E.

In the absence of direct evidence on the subject, Mr. Sharpe endeavours to prove that Arenig and Arran Mowddy are at least as modern as the Ludlow rocks, by showing that the upheaving of these chains has broken up the parallelism of the cleavage planes of the slaty rocks resting on them: assuming that these planes had originally a constant direction in each district, their dislocation at any spot would show that it had been disturbed subsequently to the cessation of the cleavage process, and we may thus class igneous eruptions as prior to, or posterior to, the cleavage; and may then connect them with the deposition of the formations, by observing at what epoch the cleavage ceased in the district. In North Wales and in Westmoreland, the cleavage only reaches into the Lower Ludlow formation; in Devonshire and Cornwall it continued later: therefore Arenig and Arran Mowddy must have been upheaved after the epoch of the Lower Ludlow shale.

The memoir concludes with a general list of the species of fossils found near Bala.

"Notice on the discovery of the Remains of Insects in the Lias of Gloucestershire, with some remarks on the Lower Members of this Formation." By the Rev. P. B. Brodie, F.G.S.

The lower beds of the lias, in which these organic remains occur, are extensively developed in the neighbourhood of Gloucester and Cheltenham, and occupy the greater part of the vale. In the upper part of the lower beds, in a hard blue limestone, was found the elytron of a coleopterous insect of the family *Buprestidæ*, apparently a species of *Ancylocheira* of Escholtz. This was the only fossil of the kind met with by Mr. Brodie in this portion of the lias. With this exception, the numerous fossil insects he has obtained occur in the bottom parts of the lower beds near the base of the lias, which are seen at several points in the neighbourhood of Gloucester. At Wainlode Cliff, the lower beds of lias, resting on red marl, form a bold escarpment on the south bank of the Severn, and afford the following section in descending order:—

1. Clay: 3 ft.
2. Blue limestone, with *Ostrea*, &c. (the "bottom bed"): 4 in.
3. Yellow shale with fucoid plants: 6 in.
4. Gray and blue limestone, termed by Mr. Brodie "insect limestone" from its characteristic fossils, passing into yellow shale above, where it is nearly white, and has the aspect of a freshwater limestone: 3 to 5 in.
5. Marly clay: 5 ft. 3 in.
6. Hard yellow limestone, with small shells like *Cyclas*, plants and *Cypris*: 6 to 8 in.
7. Marly clay: 9 ft. 6 in.
8. Bed with fucoid bodies: 1 in.
9. Shale: 1 ft. 6 in.
10. Pecten bed: 4 in.

Nine feet below this is the bone-bed, 20 feet above which is the yellow *Cypris* limestone, and 26 feet 2 inches the insect limestone. The total height of the cliff is about 100 feet.

The insect remains consist chiefly of elytra belonging to the several genera of Coleoptera, which are not very rare; and a few wings, not unlike the genus *Tipula*, which bear a close resemblance to some Mr. Brodie had previously found in the Wealden; the latter are much rarer than the former. The elytra are generally of a light brown colour and small size; in some cases both the elytra are attached. With these were found abdomens of some insects and larva apparently of the gnat tribe. Shells are not common, but *Ostrea*, *Unio*, and a small species of *Modiola* are the most abundant. The fossils from the yellow limestone, No. 4, bear a close resemblance to those from the Wealden. The real genus of the bivalve resembling *Cyclas* is undetermined. The plants belong to a species of Fucus, apparently an inhabitant of fresh water. At Combe-hill Mr. Brodie also observed both the insect limestone and that containing the small bivalves. To the south-west of this point the insect limestone is well seen, and yielded the greatest number and variety of insect remains. Here the yellow limestone was not traced, and the bone-bed was wanting. The fossil insects are, as at Wainlode Cliff, for the most part remains of small Coleoptera, sometimes tolerably preserved, and in one specimen the eyes were visible. None of the beetles resemble those of the Wealden, but some wings of insects, allied to *Tipula*, are very similar. A few imperfect but large wings of *Libellula* occur: there are also numerous singular impressions of a doubtful nature, many of which may however owe their origin to the partially decomposed bodies of various insects. With these are numerous small plants, some resembling mosses, but very different from those in the yellow *Cypris* limestone, a few seed-vessels and leaves of fern. A small species of *Modiola*, probably *M. minima*, is exceedingly abundant. Remains of Crustacea occur, one of which resembles the genus *Eryon* from the Solenhofen slate.

Near Gloucester the same strata occur at a much lower level. At Westbury, eight miles below Gloucester, the following section is presented :—

1. Bottom bed with *Ostrea*, equivalent to that at Wainlode and other places: 3 in.
2. Insect limestone with numerous small shells (here characteristic): 4 in.
3. Clay: 5 in.
4. Green, yellow and gray sandy stone, in places becoming a limestone, with the small *Cyclas*-like bivalve, plants and *Cypris*, identical with those at Wainlode, about 1 ft.
5. Shale and clay: 10 ft.
6. Hard grit, bone-bed: 3 or 4 ft.

A little further to the north the beds below this are more developed and are seen resting upon the red marl.

If the *Cypris* found in these beds be of freshwater origin, it forms a new and highly interesting feature in the history of this deposit; at any rate the occurrence of the remains of such delicate creatures as insects, many of which are well-preserved, and could not, therefore, have been long subject to the action of the waves, or have been

carried far out into the water, gives a greater probability to the supposition that this part of the lias may have been formed in an estuary which received the streams of some neighbouring lands, perhaps numerous scattered islands, and which brought down the remains of insects, *Cypris*, and the plants above referred to. The shells usually found in the insect limestone are *Modiola* and *Ostrea*, both of which frequently inhabit estuaries, and are capable of living in brackish water as well as in the open sea. The shells, however, so abundant at Westbury in the same stratum are exclusively of marine origin; the wing of a dragon-fly from Warwickshire is a solitary instance of its kind. Mr. Brodie observes, that such stray specimens had probably been carried out to sea, which might also have been the case with a small wing he discovered in the *upper lias* at Dumbleton near Tewkesbury; which also proves the existence of insects during the deposition of the upper portions of this formation.

Thus it will be seen that the remains of insects are of very rare occurrence in the upper beds, and in the higher portions of the lower ones in the lias, while at the base near its junction with the red marl they are abundantly distributed. The discovery of small elytra of coleopterous insects and portions of the wings of *Libellula* in the lower division of the lias near Evesham, by Mr. H. E. Strickland, shows that these fossils are characteristic of the same beds in distant parts of the system.

"On certain impressions on the surface of the Lias bone-bed in Gloucestershire." By H. E. Strickland, M.A., F.G.S.

The singular markings described, which the author in a former communication suggested might be caused by the crawling of crustacea, but which further opportunities and observations have induced him to refer to a different cause, have been noticed only at Wainlode Cliff on the Severn. There they occur on the uppermost surface of the band of micaceous sandstone which represents the "bone-bed," and which appears to have consisted of a fine-grained muddy sand, capable of receiving the most minute impressions, while the pure black clay which forms the superincumbent stratum has preserved this ancient surface in the most unaltered condition. The ripple-marks produced by currents on the surface of this bed of sand are very interesting, from their perfect preservation, and from often exhibiting two sets of undulations oblique to each other, indicating two successive directions in the currents, such as would result from a change of tide.

The impressed markings were evidently produced by living beings, probably by fish or invertebrate animals. To determine their nature Mr. Strickland observed the progression of two species of *Littorina* among Gasteropodous Mollusca, and of *Carcinus Mænas* among Crustacea, but the impressions produced were very different from those under consideration.

The fossil impressions are of four kinds:—

1st. Lengthened and nearly straight grooves, about one-tenth of an inch in width, and several inches long, very shallow, with a rounded bottom. These, Mr. Strickland considers as caused by

some object striking the surface of the sand with considerable impetus. They may often be seen to cut through the ridge of one ripple-mark, and after disappearing in the depressed interval, they are again seen pursuing their former direction across the next ridge. They may have been caused by fish swimming with velocity in a straight direction, and occasionally touching the bottom with the under part of their bodies.

2nd. Small irregular pits averaging one-fourth of an inch wide and one-eighth of an inch deep. These might have been caused by some small animal probing the mud and turning up the surface in quest of food. Mr. Strickland conjectures that some of the numerous species of fish found in the bone-bed may have produced them, the heterocene form of tail common to most of which, Dr. Buckland has suggested, enabled them to assume an inclined position with the mouth close to the ground.

3rd. Narrow deep grooves, about one-twelfth of an inch in width, the sides forming an angle at the bottom, irregularly curved and often making abrupt turns, apparently formed by a body pushed along by a slow and uncertain movement, such as might arise from the crawling of Mollusks. Mr. Strickland refers them to the locomotion of Acephalous Mollusca, and supposes that the only shell found in this bed, a small bivalve named by him *Pullastra arenicola*, might have produced them*.

4th. A tortuous or meandering track consisting of a slightly raised ridge about one-tenth of an inch wide, with a fine linear groove on each side. These tracks are analogous to those formed by the crawling of small annelidous worms, as may often be seen on the mud of the sea or fresh water.

About eleven feet above the stratum which presents the impressions above described, a second ossiferous bed occurs at Wainlode Cliff, which escaped Mr. Strickland's notice in the section formerly given (Geol. Proc. vol. iii. p. 586). It is a band of hard, grey, slightly calcareous stone, about an inch thick, containing a plicated shell resembling a *Cardium*, and scales and teeth of *Gyrolepis tenuistriatus*, *Saurichthys apicalis*, *Hybodus Delabechei*, *Acrodus minimus*, and *Nemacanthus monilifer*, all of which occur in the true "bone-bed" below. On the upper surface of that bed are numerous impressions, termed by Mr. Strickland fucoid, consisting of lengthened wrinkled grooves, variously curved, about three quarters of an inch wide, one-eighth of an inch deep, and of variable length. The bone-bed seems to be a local deposit, not being met with in the other localities examined by the author, and being confined to a portion only of Wainlode Cliff, where it constitutes No. 9. in the following corrected section :—

* Mr. Strickland describes this species as follows :—"Its form is nearly a perfect oval, depressed, nearly smooth, but with faint concentric striations towards the margin. The apex is about halfway between the middle of the shell and the anterior end. The general outline closely resembles that of the recent *Pullastra aurea* of Britain. Maximum length 7 lines, breadth $4\frac{1}{2}$ lines, but the ordinary size is less."

	Ft.	in.
1. Blackish lias clay	3	6
2. Limestone, with *Ostrea* and *Modiola minima* (the bottom bed)	0	4
3. Yellowish shale	1	0
4. Limestone, with remains of insects	0	4
5. Marly shale and clay	5	3
6. Yellowish limestone nodules, with occasional remains of *Cypris*	0	6
7. Yellowish marly clay	6	0
8. Black laminated clay	3	6
9. Stone, with scales and bones of fish, and on the upper surface fucoid impressions	0	1
10. Black laminated clay	1	6
11. Slaty calcareous stone, with Pectens	0	4
12. Black laminated clay	9	0
13. Bone-bed and white sandstone, with casts of *Pullastra arenicola*	0	3
14. Black laminated clay	2	0
15. Greenish angular marl	23	0
16. Red marls with greenish zones	42	0
	98	7

December 14, 1842.—On the Ridges, Elevated Beaches, Inland Cliffs and Boulder Formations of the Canadian Lakes and Valley of St. Lawrence. By Charles Lyell, Esq., V.P.G.S., F.R.S.

January 4, 1843.—The reading of Mr. Lyell's memoir, commenced on the 14th of December, was resumed. An abstract of it has already been given, see p. 518, note.

Notice on a Suite of specimens of Ornithoidicnites, or foot-prints of Birds on the New Red Sandstone of Connecticut." By Gideon Algernon Mantell, LL.D., F.R.S.*

Extract of a Letter from W. C. Redfield, Esq., on newly discovered Ichthyolites in the New Red Sandstone of New Jersey. Communicated by Charles Lyell, Esq., V.P.G.S.*

A Letter was read from Mr. Charles Nicholson, accompanying some fossil bones found imbedded in the banks of the Brisbane River (New South Wales).

Also an extract of a Letter from his Excellency George Grey, Governor of Adelaide, to Mr. Lyell, accompanying a section of the country between the eastern shore of St. Vincent's Gulf and Lake Alexandrina (New South Wales), and noticing some fossils obtained from that district.

January 18th, 1843.—" On the Silurian Rocks of the South of Westmoreland and North of Lancashire." By Daniel Sharpe, Esq., F.G.S.

This communication is in continuation of a paper read by the author on the 2nd of February, 1842†, a second visit to the district

* Abstracts of these papers have already been given, see p. 518, note.

† See Phil. Mag. S. 3. vol. xxi. p. 555.

having enabled him to correct some errors committed on his first examination, and to extend his observations into Lancashire.

On both occasions Mr. Sharpe took for his base-line the bed of Coniston limestone described by Professor Sedgwick*, being convinced that Mr. Marshall has rightly considered that limestone as the lowest bed of the Silurian system in this district†, and in all his descriptions he adheres to the ascending order.

1st. *Coniston Limestone*.—It is doubtful whether this bed is continuous at its western extremity, or occurs only in detached patches. The two western portions of limestone at Water Blain and Low House are a mile and a quarter south of the bearing of the line of the bed east of the latter place, but are exactly on a line with the strike of the bed beyond Coniston; a great fault between Low House and Greystone House being counterbalanced by the whole of the smaller faults between that spot and Coniston, which are pointed out in Professor Sedgwick's memoir. Mr. Sharpe gives a list of fossils collected in this bed and the shales above it at Torver Fell, Coniston, Long Sleddale, &c., in which are several of the species of *Orthis*, *Spirifer*, and *Leptæna*, found by Mr. Murchison in the Lower Silurian deposits, and several undescribed species.

2nd. *Slates, Shales, and Flagstones*.—These are well exposed on Torver Fell, where the following series may be seen:—

a. Brown shale.

b. Dark blue slate of good quality; the beds dip E.S.E. 40°, and the cleavage dips S.S.E. 80°; it contains many fossils, much compressed and distorted, nevertheless a few Lower Silurian shells are made out.

c. Indurated brown shale.

d. Blue flagstone rock, a bed well known in the district, and mentioned by Professor Sedgwick and Mr. Marshall; at Torver, where it gives good roofing-slate as well as flags, the beds dip south-east 45°, and the cleavage south-east 80°. To the eastward of Windermere this bed and the lower bed of slate *b* run together, and the whole of the Lower Silurian formation diminishes in thickness.

e. Indurated shale.

f. Shear Bed, which supplies brownish-blue flags, taken along the bedding of the rocks, which is free from slaty cleavage.

This series of slates, flagstones, and shales, may be traced above the Coniston limestone from the Dudden to Shap Fells, although the separate beds cannot always be distinguished.

3rd. *Grey Slaty Grits*, described in Mr. Sharpe's former paper as the "Lower division of the Windermere rocks," but now classed as part of the Lower Silurian formation; they consist of a great thickness of hard gritty grauwacke, variously affected by cleavage, and may be traced from the Dudden, below Broughton, to Shap Fell.

4th. *Blawith Limestone*, "the second band of calcareous slate"

* Geol. Trans. Second Series, vol. iv. p. 47.

† Report of the British Association, 1839, Sections, p. 67.

of Professor Sedgwick; a bed only found in two localities, at Meer Beck and a wood behind Low Hall, on the east of the road from Ireleth to Kirkby Ireleth, where it is a dark-blue limestone very like that of Coniston, dipping east 40°, of which only about a thickness of twelve feet is laid open; and at Turtle-bank Heights, south-west of Blawith, where it has been quarried near the top of the south-east face of the hill, and is a dark gray limestone, twenty feet thick, striking north-east and dipping perpendicularly; from this spot it runs by Cockin's-hill to the side of Coniston Water, half a mile north of Water Gate. The fossils found by Mr. Marshall in this bed near Blawith were identified as Lower Silurian species.

5th. *Flagstones and Slates of Kirkby Ireleth.*—These are placed by Professor Sedgwick below the Blawith limestone, No. 4, but as Mr. Sharpe considers erroneously: nevertheless, although no fossils have been found in them, he considers them to be the uppermost bed of the Lower Silurian series, because they are always conformable to the undoubted Lower Silurian beds below them, and are not equally conformable to the beds above. As this southern edge forms the boundary line of the Lower Silurian formation, Mr. Sharpe traced them carefully along their whole course, from their first appearance rising from under the mountain limestone, on the east of Ireleth, till they are hidden by the old red sandstone of Birkbeck-beck. Near Ireleth it is only used for building-stone, but at Kirkby Ireleth are quarries extending for a mile and a half along the range of the bed, supplying dark-blue slates of very good quality. At Horse Spital Quarry the beds dip south-east 80°, and the cleavage dips south-east 55°, both sets of planes striking north-east: this coincidence in the strike of the bedding and cleavage planes is common in all this district; yet at Lord Quarry, close to the last-mentioned, the beds dip N.N.E. 20°, while the cleavage dips S.S.E. 70°. Further east the rock is of inferior quality, and is rarely worked for roofing-slate: its usual course is north-east, passing by Suberthwaite, Blawith, Nibthwaite, at the foot of Coniston Water, where much building-stone has been quarried, and the rock is well exposed, being a dark-blue flagstone streaked with gray: between Oxen Park and Satterthwaite it dips north 50°, and N.N.W. 70°, and is lighter and more striped than usual; at Force Mill it strikes E.N.E. and dips N.N.W. 65°, and the cleavage has the same strike but is perpendicular: at Satterthwaite the dip is north 45°: between Esthwaite and the Ferry on Windermere the road runs near the upper edge of the bed, which is well exposed close to the Ferry House, north of which spot it reaches more than a mile up the shore of the lake. On the east side of the lake it has been quarried north of Bowness.

Eastward of Bowness, Mr. Sharpe corrects an error which he committed in laying down this line too far south: he now traces it nearly E.N.E. by Ing's Chapel, Row Gill, and Hugill Hall, dip south-east 60°; Monument Hill on the west side of Kentmere, dip S.S.E. 80° to Fellfoot in Kentmere. The flagstone crosses Long Sleddale at the Chapel, where it was found not worth working for slate: at Bonnisdale-head Farm it gives a slate of fair quality, the

beds dip south-east by south 65°, and the cleavage dips in the same direction 80°; from here it crosses into High Borrowdale half a mile above High House, dipping south-east by south 50°; a fault down this valley throws the bed below High House on the east side of the valley: in the next Fells it is much concealed by the vegetation, but it is seen at a cutting of the road from Shap to Kendal on Hurd's Brow, between the ninth and tenth milestone, dipping south-east 75°, and the cleavage dipping north-west 85°. Near the Borrow the beds are thrown into several anticlinal ridges bearing north-east, by faults which disturb the cleavage planes as well as the bedding of the rock: this slate has also been worked in the upper part of Bretherdale. The boundary thus laid down nearly corresponds with that given in the new edition of Mr. Greenough's map.

The lowest beds of the slate in High Borrowdale are calcareous, and may perhaps represent the Blawith limestone, which has not been found in conjunction with the slate eastward of Blawith.

In High Furness, the district of Lancashire consisting of Lower Silurian rocks, the principal valleys run from south-west to north-east, parallel to the strike of the beds, each ridge of hills representing the outcrop of a particular bed: this is not the case with the same formation in Westmoreland, where the valleys of Coniston Water, Esthwaite, Windermere, Troutbuk, Kentmere, Long Sleddale, Bannisdale, High Borrowdale, and Brethesdale, all follow great faults across the strike of the stratification: these faults are continued through the Windermere rocks, and sometimes into the Lower Ludlow rocks, but are lost before entering the Upper Ludlows.

It is in High Furness that the Lower Silurian formation is best exposed to observation, and has a greater thickness than in Westmoreland, the beds gradually diminishing in their course eastward. In the same district of Lancashire the slaty character of the rocks is more developed than we find it in Westmoreland; it is especially between Coniston, Old Mere and Kirkby Ireleth, that the crystallizing agency which has changed the rocks into slate has acted most powerfully, many beds in that district supplying good slate, which will hardly split up at all elsewhere.

From the prevailing parallelism long known to exist between the planes of slaty cleavage over considerable areas, Mr. Sharpe considers it nearly certain that these planes had a uniform direction in each district, and that the cases of exceptions which are found are due to disturbing forces acting after the cessation of the cleavage action. In the district under consideration the mean dip of the cleavage planes is considered to be S.S.E. 70°, and the cleavage action is thought to have ceased before the formation of the Upper Ludlow rocks.

Windermere Rocks.—The beds formerly classed by the author as the lowest division of this series are now placed in the Lower Silurian formation, and the middle and upper divisions are thrown together, for want of any distinct line of division between them, and some considerable corrections are made in their geographical boundaries. They rise, near Ulverston, from below the mountain limestone of

Low Furness, dipping E.S.E. at high angles, and disappear in Westmoreland beyond Bannisdale, during which course they rest on the Kirkby Ireleth slate; but their southern boundary can only be understood from the map, as to the west of Windermere they are overlaid by large patches of mountain limestone, and in their range eastward are gradually covered up unconformably, and concealed by the Lower Ludlow rocks. In some places the similarity of the rocks of the two formations, and the absence of fossils in both, makes it difficult to determine the boundary between them, the best guide being the dip and strike of the rocks. In Mr. Sharpe's first map a portion of the Lower Ludlow rocks on the north-east of Kendal was erroneously coloured as belonging to the Windermere series; the error was pointed out by Cornelius Nicholson, Esq., of Cowan Head, who assisted the author materially in mapping the neighbourhood of his residence.

The upper boundary of the Windermere rocks begins on the south-west at the lower point of Witherslack, and is marked by a great fault which crosses the valley between that hill and Whitbarrow, and appears to pass under the mountain limestone of Whitbarrow, then runs north-east through Underbarrow, by the Chapel, to Mountjoy: on the west side of this fault the Windermere rocks form high ridges of hard slaty grits of dark grey colour, with lighter streaks, dipping N.N.W., while on the east side of the fault is a gritty rock of uniform grey colour dipping E.S.E., overlaid with beds containing the fossils of the Ludlow beds. From Mountjoy the line turns to the north-west, and passes round Crook Chapel, which stands on a ridge of the Windermere grits; at Crook Common it turns to the north-east, and follows that direction to near Borrowdale, where the formation is lost, being completely hidden by the Ludlow rocks, which there rest on the Lower Silurians. Crook Common is thrown into great confusion by the meeting of two lines of elevation, one coinciding with the E.N.E. strike of the Lower Silurian rocks, the other coming up from the S.S.W. through Cartmel Fell.

At Backbarrow, below Newby Bridge, the upper beds of this series are slaty, with a wavy cleavage dipping N.N.E. 80°, the beds dipping south-east 80°; these beds contain irregular calcareous nodules in great abundance, and *Orthoceras articulatum* was found in them.

Mr. Sharpe refers to his former memoir for the description of the Windermere rocks on the east of the Lune, which extend to Greyrigg Forest, Whin Fell, and Howgill Fell; in these Fells are several axes of elevation which require further examination.

Ludlow rocks.—These were described in the author's former paper; the area covered by them is larger than was there stated, their lower boundary being now carried more to the north, and their eastern portion being extended in a sort of trough between the Lower Silurian slates of Shap Fell and the Windermere rocks of Whin Fell, crossing Barrowdale between High and Low Barrowbridge.

In the lowest beds of the series in Fawcett Forest were found *Leptæna lata* and *Turritella conica*, in a slaty rock. The *Terebratula navicula* is found thinly scattered throughout all the lower part of the

formation, and occurs in vast numbers in a bed which forms about the middle of the Ludlow series. Mr. Murchison has told us that this little shell is usually found in such numbers as to form a bed which lies above the Aymestry limestone, and it serves to mark the place of that rock where it is wanting: and Mr. J. E. Davis informed the author, that at Stapleton, near Presteign, where there is no Aymestry limestone, this species is found throughout the whole of the Lower Ludlow shales. Mr. Sharpe has made use of this shell in dividing the Upper from the Lower Ludlow rocks in Westmoreland, classing all the beds containing it in the lower series. The bed in which it occurs in greatest abundance was traced through Underbarrow, by Tullithwaite Hall and High Cray, across the west end of Rather Heath and a little south of Cowan Head, and also in Lambrigg Park; it is usually accompanied by *Atrypa affinis, Spirifer octoplicatus, Leptæna lata* and *depressa, Orthis lunata*, and *Terebratula nucula*: the *T. navicula* seems to have died out suddenly, as it is not found in the Upper Ludlow beds.

The same division of the Ludlow rocks may be obtained by attending to the direction and dip of the beds; the lower series partakes of the north-east strike, which runs through the older Silurian rocks in these counties, and is traversed by many of the same faults as those formations, but the Upper Ludlow beds are thrown up in anticlinal ridges with a different direction.

Mr. Sharpe gives a list of the organic remains found in each division of the formation, which includes forty-four of the species described in Mr. Murchison's work from the old red sandstone and Upper Ludlow, fourteen of those from the Aymestry limestone, and twenty-two of those from the Lower Ludlow beds. Of the species of shells placed by Mr. Murchison in the old red sandstone*, all but two have now been found low in the Ludlow beds, proving that the red beds containing these species in Herefordshire must be classed with the Upper Ludlow formation.

Old Red Sandstone.—The only addition to the former paper which relates to this formation, is in mapping it in the upper valley of the Lune, where the tile-stones reach above the hamlet of Langdale, dipping N.N.E. 10°.

The age of the large masses of gravel of a brown or red colour noticed in the valley of the Lune between Sedberg and Casterton, and of the Kent and Sprint, was before left uncertain; the author now regards them as a modern surface drift.

Mountain Limestone.—The description of this formation did not enter into Mr. Sharpe's plan, but he examined the portion of it which occurs in Low Furness, to ascertain the geological position of the Ulverston iron ore.

The ore occurs in veins usually perpendicular, and bearing W.N.W., which cut through the limestone, but are not continued into the Silurian rocks. The following veins are mentioned:—

Plumpton Hall; now abandoned.

Lindal Moor vein; an exception to the usual condition, as it runs between the mountain limestone and the Windermere grits, striking

* Silurian System, p. 603. and t. 3.

north-west and dipping south-west 45°; it is the principal and most profitable vein of the district.

Stainton; three veins separated hy a few yards of clay, spar, and limestone, perpendicular, and bearing W.N.W.

Lindal Court; several perpendicular veins near together, bearing W.N.W.

Crosthwaite; a poor vein bearing W.N.W., thought to be the continuation of that at Stainton.

Wet Flat; the rocks near are much disturbed, and the vein, after running W.N.W., turns down a fault in the limestone to N.N.W., but soon thins out.

Trap Rocks.—These are rare in the district; Professor Sedgwick has laid down some masses of igneous rocks at Shap Fells, on the south side of the high road; one of them consists of red felspar with some mica, quartz, and hornblende. The slate rocks are much disturbed in the neighbourhood, and the faults have broken up the cleavage planes as well as the bedding of the rocks, from which Mr. Sharpe infers that the trap is more modern than the eruption of the Shap granite, which took place before the cleaving of the slates, as the cleavage planes run through all the faults connected with that eruption.

At Biglands, south of Newby Bridge, there is a trap dyke running north-east, which has also disturbed the parallelism of the cleavage, and must he considered as of a modern date: it is not well exposed on the surface.

The author concludes by a comparison of all the heds with those described hy Mr. Murchison in the border counties of Wales, and adopted as the types of the Silurian system, and with those of Denbighshire and Merionethshire, to which his attention was directed by Mr. Bowman's papers on Llangollen; he points out the closest resemblance between the Silurian formation in North Wales and in Westmoreland, while in mineral character they differ most materially from those of Siluria: nevertheless the principal divisions of the Silurian system laid down by Mr. Murchison can be traced in each district hy the evidence of the organic remains.

"On the Stratified Rocks of Berwickshire and their imbedded Organic Remains." By Mr. William Stevenson, of Dunse. Communicated by the President.

In this memoir the author gives an account of the characteristic features, the order of succession, and the nature of the organic remains of the stratified rocks of Berwickshire. The lowest of these are greywacke and greywacke slate, forming an extensive system of arenaceous and argillaceous strata of various colours, gray predominating, found almost everywhere among the Lammermuirs, of which chain they constitute the fundamental rock. In the rocks of this system no undoubted organic remains have heen found, but some curious markings occur on slabs, for which it is difficult to account without supposing the influence of organic agency. The greywacke presents the uniform appearance of a deep sea deposit, perhaps laid down upon the hottom of a wide-spreading ocean of great profundity, and therefore removed from the disturbing action of wind and tides. The thickness of these strata, as displayed among

the Lammermuirs, is very great, but the series is far from being complete, there being no appearance of the older strata on the one hand, and on the other their junction with the newer formations is always unconformable. The materials of which they are composed were probably derived from the disintegration of the granites and primary schists to the westward.

2. The formation next in order is the upper division of the old red sandstone, the members of which rest unconformably upon the upturned ends of the greywacke. The lowest member of it is an old red sandstone conglomerate, consisting of fragments of greywacke and felspathic rocks, cemented by a paste which is generally arenaceous, sometimes calcareous. It varies much in thickness.

3. Red and greenish white sandstones succeed with soft red argillaceous strata. Part of these seem to have been formed in a shallow sea, since they exhibit ripple-marks, and contain remains of *Holoptychius* and *Dendrodus*. Another portion contains few traces of fossils, and was probably deposited in deeper water. Some curious spindle-shaped concretions and the impressions called Kelpie's feet occur, also traces of *Fuci*. Two localities near Preston-Haugh, and one at the foot of the Knock-hill, are all in which organic remains have as yet been found.

4. After the deposition of the strata containing the remains of the *Holoptychius*, &c., a subsidence to a considerable extent took place, after which a succession of strata of great thickness was deposited above them. These rocks seem to have been formed in deeper water than the ichthyolitic beds. They consist of red and greenish white sandstones interstratified with beds of a softer and more argillaceous character, and of a deep red colour. They seem to contain no organic remains except vegetable impressions (Algæ?) which occur in abundance in a bed of red sandstone, perhaps 100 feet above the strata containing the animal remains.

5. Above the soft, red and white sandstones are calcareous shales, sandstones and cornstones, or impure concretionary limestones, without fossils. The junction of these with the sandstones is not seen, being cut off by faults and trap dykes.

6. The lower portion of the coal-measures succeeds, consisting of shales, marls, clays, and sandstones containing ironstone bands and gypsum, and abounding in vegetable fossils, consisting of *Coniferæ*, *Stigmariæ*, *Lepidodendra*, and other coal plants. This formation is well developed over the greater part of the Merse of Berwickshire.

7. Next in order are some thick beds of reddish sandstone, underlying

8. Carboniferous strata, consisting of sandstones, shales, &c., including three or four coal-seams.

9. The encrinal limestone, seen a little north of Berwick.

Mr. Stevenson remarks that the Berwickshire carboniferous strata appear to correspond with the lower beds of the Fife and Lothian coal-fields, considered by Mr. Milne and others to belong to the mountain limestone, and to be considerably lower than the Newcastle coal strata. With regard to the inquiry whether new red sandstone exists in Berwickshire, Mr. Stevenson is inclined to

answer it in the negative. He regards the beds at Cumledge, described by Mr. Milne as such, as old red, and considers the soft red clays and sands at Lintlaw, derived from the disintegration of the old red sandstone, referred by Mr. Milne to the new red sandstone, to be of undetermined age, from want of sufficient evidence in the absence of organic remains. The exact position of the greywacke strata of the Lammermuirs is for the same reason indeterminate. The author concludes by pointing out the great gap which occurs between the greywacke and the upper division of the old red sandstone in Berwickshire, the middle and lower divisions of the old red and the whole of the Silurian system being deficient. Another circumstance worthy of remark is the absence of any formations more recent than the coal-measures, if we except alluvial deposits and the undetermined red strata formerly mentioned.

February 1, 1843.—A paper was read "On the Tertiary Strata of the Island of Martha's Vineyard in Massachusets." By Charles Lyell, Esq., V.P.G.S., &c.*

Letter from J. Hamilton Cooper, Esq., to Charles Lyell, Esq. V.P.G.S., "On Fossil bones found in digging the New Brunswick Canal in Georgia*.

"Description of some Fossil Fruits from the Chalk-formation of the South-east of England." By Gideon Algernon Mantell, LL.D., F.R.S., &c.

The fruits described are three in number, viz.—

1. *Zamia Sussexiensis*, Mantell.—From the greensand. A cone allied to the *Zamia macrocephala*, a greensand fossil from Kent, figured in Lindley and Hutton's 'Fossil Flora,' pl. 125, from which it differs in form and in the number, size, and shape of its scales, which are more numerous, smaller and more oblong than in the Kentish species. It is five inches long, and at the greatest circumference measures six inches. It was found about two years ago in an accumulation of fossil coniferous wood in a sand-bank at Selmeston, Sussex, at the junction of the Shanklin sand with the gault. Dr. Mantell having sent a cast of the only specimen found to M. Adolphe Brongniart, that distinguished botanist suggested that it might be either the stem of a young cycadaceous plant or the fruit of a *Zamia*, but the situation and small size of the stalk at the base and the appearance of the scales, induce Dr. Mantell to refer it to the latter.

2. *Abies Benstedi*, Mantell.—From the greensand near Maidstone, Kent. A beautiful cone found by Mr. W. H. Bensted in the quarry in which the remains of the Iguanodon were discovered in 1834, where it was associated with *Fucus Targionii*, and some indeterminate species of the same genus; stems and apparently traces of the foliage of endogenous trees allied to the *Dracæna* (*Sternbergia*), and of trunks and branches of *Coniferæ*. The wood occurs both in a calcareous and siliceous state. The cone found is in every respect such a fruit as the trees to which the wood belonged might have borne. It bears a close resemblance to a fossil from the greensand

* Abstracts of these papers have appeared in the present volume; see p. 518, note.

of Dorsetshire, discovered by Dr. Buckland, and figured in the 'Fossil Flora' of Great Britain under the name of *Abies oblonga* (Fos. Fl. pl. 1.). Unfortunately the outer surface is so much worn that the external figure of the scales cannot be accurately defined; but the sections show their proportionate thickness. There is an opening at the base of the cone occasioned by the removal of the stalk, and an accidental oblique fracture exhibits the internal structure. In the longitudinal section thus exposed the scales are seen to be rounded and broad at their base and to rise gradually, and become thin at their outer terminations. The seeds are oblong, and one seed is seen imbedded within the base of each scale. Mr. Morris considers it to have a great affinity to *Abies oblonga* of Lindley and Hutton, but it is more spherical, and the scales are smaller, more regular and numerous.

3. *Carpolithes Smithiæ*, Mantell.—From the white chalk of Kent. An account of an imperfect specimen of this fruit was formerly given by Dr. Mantell in his 'Illustrations of the Geology of Sussex.' He lately detected a second and more perfect example in the choice collection of Mrs. Smith of Tunbridge Wells, in honour of whom he has named it. Dr. Mantell remarks, that a slight inspection was sufficient to determine its vegetable origin, for several seeds were imbedded in its substance, and others had been detached in clearing it from the chalk. Dr. Robert Brown suggested that the original was probably a succulent compound berry, the seeds appearing to have been imbedded in a pulpy substance like the fruit of the mulberry, which is a spurious compound berry, formed by a partial union of the enlarged and fleshy calices, each inclosing a dry membranous pericarp.

From the occurrence of the cones above described with the drifted remains of land and freshwater reptiles peculiar to the Wealden, Dr. Mantell infers that these fruits belong to the flora of the country of the *Iguanodon*.

"Notice on the fossilized remains of the soft parts of Mollusca." By Gideon Algernon Mantell, LL.D., F.R.S., &c.

Substances presenting the same general appearance and composition with coprolites, but destitute of the spiral structure, are thickly interspersed among the shells which abound in the rocks of firestone or upper greensand at Southborne in Sussex, sometimes occurring in the state of casts of shells of the genera *Cucullæa*, *Venus*, *Trochus*, *Rostellaria*, &c., from the soft bodies of which testacea Dr. Mantell considers them to have originated. They abound also in the layers of firestone which form the line of junction with the gault, and are not uncommon in the gault itself in several localities in Surrey and Kent.

Dr. Fitton, in his memoir 'On the Strata below the Chalk' (Geol. Trans. vol. iv. part 2. p. 11), has given an account of similar concretions from Folkstone, where he observed them in some cases surrounding or incorporated with fossil remains, and filling the interior of Ammonites. Dr. Mantell has observed them also in the Shanklin sand in Western Sussex, in Surrey, near Ventnor in the Isle of Wight, and in Kent, and they especially abound in the Igua-

nodon quarry of Kentish rag near Maidstone, belonging to Mr. W. H. Bensted.

Mr. Bensted having long paid attention to this subject, more than two years ago submitted to Dr. Mantell specimens of fossil shells, the cavities of which were filled with a dark brown substance in every respect identical with the nodular and irregular concretions of coprolitic matter which abound in the surrounding sandstone. Mr. Bensted expressed his belief that the carbonaceous substance was derived from the soft bodies of the Mollusca, and that the concretionary and amorphous portions of the same matter dispersed throughout the sandstone of this bed, were masses of the fossilized bodies of the animals which had become disengaged from their shells, and had floated in the sea till enveloped in the sand and mud, which is now concreted to the coarse sandstone called Kentish Rag. In proof of this opinion reference is made to an account published in the 'American Journal of Science' for 1837, of the effects of an epidemic among the shell-fish of the Ohio, which, killing the animals, their decomposed bodies rose to the surface of the water, leaving the shells in the bed of the stream, and floating away covered the banks of the river. Mr. Bensted points out that nearly the whole of the shells in the Kentish rag of his quarry appear to have been dead shells, and infers that their death might have been owing to a similar cause with that which destroyed the *Uniones* in America; while their bodies intermingling with the drift wood on a sand-bank furnished the concretions described in this communication.

The Rev. J. B. Reade submitted some of the substance of these bodies to an analysis by Mr. Rigg, who confirmed Dr. Mantell's suspicion of the presence of animal carbon in it, and states that the darker portion of the substance contains about 35 per cent. of its weight of carbon in an organized state.

Dr. Mantell adds, that a microscopical examination with a low power detects innumerable portions of the periosteum and nacreous laminæ of the shells of extreme thinness intermingled with the carbonaceous matter, together with numerous siliceous spiculæ of sponges, very minute spines of *Echinodermata*, and fragments of *Polyparia*, and remarks that these extraneous bodies probably became intermingled among the soft animal mass before the latter had undergone decomposition. He proposes to term the substance *Molluskite*, and states that it constitutes the dark spots and markings in the Sussex and Purbeck marbles.

"On the Geological position of the *Mastodon giganteum* and associated fossil remains at Bigbone Lick, Kentucky, and other localities in the United States and Canada." By Charles Lyell, Esq., V.P.G.S.*

LXV. *Intelligence and Miscellaneous Articles.*

ON THE PHOSPHORESCENCE OF THE GLOW-WORM.

M. MATTEUCCI has performed numerous experiments on glow-worms, and has arrived at the following conclusions,

* We have already given an abstract of this paper, see note, p. 518.

some of which, he observes, are new in part, and others more accurately determined than heretofore :—

1. The phosphorescence of a glow-worm may cease before its death.

2. There exists in the glow-worm a substance which emits light, unaccompanied by sensible heat, and this does not require for its exhibition either the integrity or the life of the animal.

3. The phosphorescence of the insect ceases in carbonic acid and hydrogen gases in thirty or forty minutes, provided the gases are pure.

4. The light of the phosphorescent matter is decidedly brighter in oxygen gas than in atmospheric air, and it preserves its brilliancy for nearly three times as long. This occurs not only with the entire insect, but with the luminous segments separated from it.

5. The phosphorescent matter, when made to shine either in oxygen or in the air, consumes a portion of oxygen, which is replaced by an equal volume of carbonic acid gas.

6. The phosphorescent matter, when in contact with oxygen, but reduced to a state in which it cannot emit light, does not sensibly absorb oxygen, nor does it develope carbonic acid.

7. One proportion of oxygen and nine proportions of hydrogen or carbonic acid gas, form a mixture in which the phosphorescence continues for some hours; it may therefore be concluded that it is on account of the alteration which happens to the phosphorescent substance, that at the expiration of some days it ceases to shine, after having been put into pure oxygen, a portion of which is eventually replaced by carbonic acid gas. The hydrogen in which several glow-worms were placed for twenty-four hours was analysed, the insects having shone for a few minutes only. The same happened if the gas was pure, in operating over mercury, carefully filling the receiver and reversing it two or three times to remove the air which adheres to the glow-worms. In this hydrogen it was found that its volume was slightly augmented; with 8 cubic centimetres of hydrogen there was an increase of $0^{c.c.}2$ of volume which was absorbed by potash; it was therefore carbonic acid which the insects had produced, and this occurred either because some oxygen remained in their tracheæ, which combined with carbon and converted it into carbonic acid, or because the insects contained this acid ready formed. When the luminous segments were alone carefully put into hydrogen, they continued to shine for a few seconds only, and the gas suffered no change.

8. Heat, to a certain degree, increased the light of the phosphorescent matter; cooling produced the contrary effect.

9. When the heat is too strong the phosphorescent substance is altered, and the same occurs whether it be left in the air or in some gas for a certain time, provided it be separated from the animal.

10. This phosphorescent matter thus altered is not capable of emitting light or of becoming luminous; these facts evidently determine the nature of the phænomenon; the production of light in this insect is entirely dependent upon the combination of oxygen with the carbon, which is one of the elements of the phosphorescent matter.—*Ann. de Ch. et de Phys.* S 3. ix. 71.

ACTION OF POTASSIUM AND SODIUM ON SULPHUROUS ACID. BY MM. FORDOS AND GELIS.

When potassium or sodium is thrown into an aqueous solution of sulphurous acid, they act upon it in the same way as on pure water; potash and soda are formed and hydrogen is evolved, which inflames; the alkalies combining with the sulphurous acid to form sulphites, which remain in solution; if the experiment be made in a tube with the pure metals, the phænomena are similar, hydrogen and sulphites being obtained.

The reaction takes place with so much violence, and the rise of temperature is so considerable, that it is natural to suppose that these two circumstances influence the results, and that if the reaction were less vivid different results would be obtained; for such bodies as combine at common temperatures do not act upon each other when the temperature is raised.

MM. Fordos and Gélis endeavoured therefore to bring potassium into contact with aqueous sulphurous acid, under such circumstances as should not raise the temperature, and they succeeded in the attempt, by operating with freezing mixtures and treating sulphurous acid with potassium which had been previously combined with metals that were incapable of decomposing water or sulphurous acid by themselves; or in other words, they used the alloy of potassium and antimony, and that of potassium and mercury.

These alloys decompose water which has been well cooled, regularly and without inflammation; when they are treated with very dilute sulphuric acid containing sulphurous acid, hydrogen mixed with sulphuretted hydrogen is disengaged, the presence of which is ascertained by the smell and its action upon acetate of lead.

If these alloys be treated with water containing sulphurous acid only, hydrogen is still disengaged, for the action cannot be so regulated as to obtain the perfect reduction of the sulphurous acid; but no sulphuretted hydrogen is evolved, and acids precipitate sulphur in abundance from the solution; consequently there are formed, under these circumstances, both a sulphite and a hyposulphite.—*Journ. de Ph. et de Ch.*, Octobre 1843.

ACTION OF ZINC ON SULPHUROUS ACID, SULPHITE OF ZINC. BY MM. FORDOS AND GELIS.

An aqueous solution of sulphurous acid readily attacks zinc, especially when the metal is in filings and the solution is concentrated; there is increase of temperature but no gaseous product. Fourcroy and Vauquelin have stated, that when the action is rapid a notable quantity of hydrosulphuric acid gas is evolved; but it is easy to prove that when this occurs it is from a totally different cause from that which they assign to it. Well-washed and recently prepared sulphurous acid never produces this effect; it occurs, on the contrary, when the acid employed contains sulphuric acid.

In order to obtain a concentrated solution of zinc in sulphurous

acid, the gas, well-washed, should be passed through Woulf's bottles, containing distilled water and cuttings of zinc. In this operation the following appearances occur: the metal at first tarnishes and is covered with a grayish crust; the liquor then becomes slightly yellow, but not turbid; the colour increases until it becomes as deep as that of a concentrated solution of chromate of potash, and it continues as long as there is great excess of sulphurous acid in the liquid. If the disengagement of gas slackens, or if by the increase of temperature the metal is more rapidly dissolved, the colour diminishes, and in the first case the liquid becomes turbid and a white pulverulent deposit is formed; if the operation be now stopped, or if the liquor be suffered to remain at rest during a night, this white powder is converted into white brilliant prismatic crystals, which collect on the sides of the vessel and on the undissolved portions of the metal.

Examination of these Crystals.—They are easily obtained in considerable quantity, either by spontaneous evaporation or cautious evaporation in a water-bath; much sulphurous acid is evolved, and the surface of the solution is covered with a thick layer of crystals. These crystals may be washed with water, for they are almost insoluble in it; but water containing sulphurous acid dissolves them readily, without becoming coloured; these crystals are colourless, inodorous, transparent and insoluble in alcohol; acids decompose them with the evolution of sulphurous acid, without any deposit of sulphur; the solution in hydrochloric acid gives no precipitate with chloride of barium. When the crystals are moist they are readily converted into sulphate by exposure to the air, but when dry they may be long kept without alteration.

The preceding facts prove that these crystals consist of sulphurous acid, oxide of zinc and water; to analyse them the oxide of zinc was obtained by calcination, the sulphurous acid by converting into sulphuric by means of iodine, and noting the quantity absorbed, and the water by calculation; this it would be almost impossible to obtain directly, for the sulphurous acid is disengaged at about the same temperature.

The salt appeared to be composed of

One equivalent of sulphurous acid	32
One equivalent of oxide of zinc	40
Two equivalents of water	18
Equivalent	90

Examination of the Mother-water.—The solution from which the sulphite of zinc has been separated is colourless, transparent and inodorous, contains no sulphuric acid; and the examination proved that when sulphurous acid acts upon zinc two salts only are formed, the sulphite and hyposulphite; when, however, the mother-water is further evaporated, it yields different products according to the temperature at which it is effected, yielding sulphurous acid, sulphite of zinc and other products.—*Journ. de Ph. et de Ch.*, Octobre 1843.

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END OF THE TWENTY-THIRD VOLUME.

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